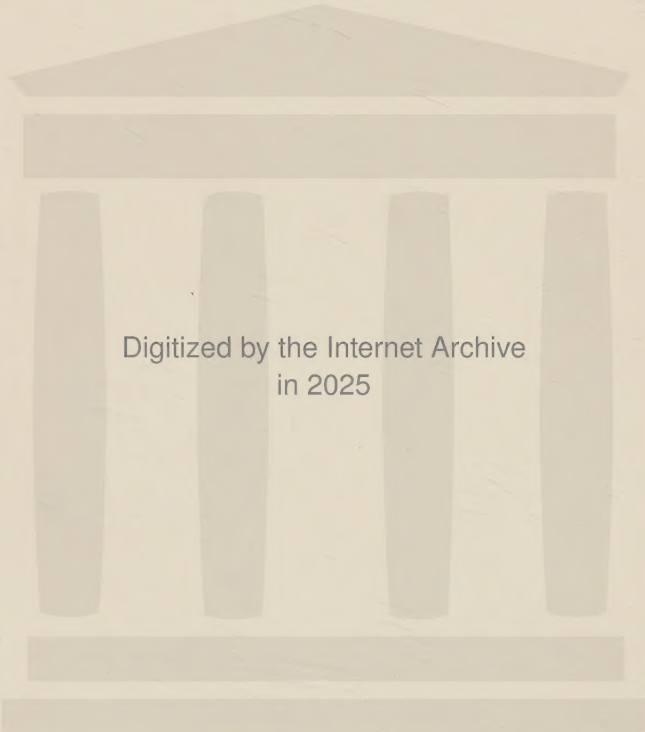


HISTORY, PHILOSOPHY AND
SOCIOLOGY OF SCIENCE



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HISTORY, PHILOSOPHY AND
SOCIOLOGY OF SCIENCE

Classics, Staples and Precursors

HISTORY, PHILOSOPHY AND
SOCIOLOGY OF SCIENCE

Classics, Staples and Precursors

Selected By

YEHUDA ELKANA
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ALFRED RUSSEL WALLACE

LETTERS AND REMINISCENCES

BY
JAMES MARCHANT



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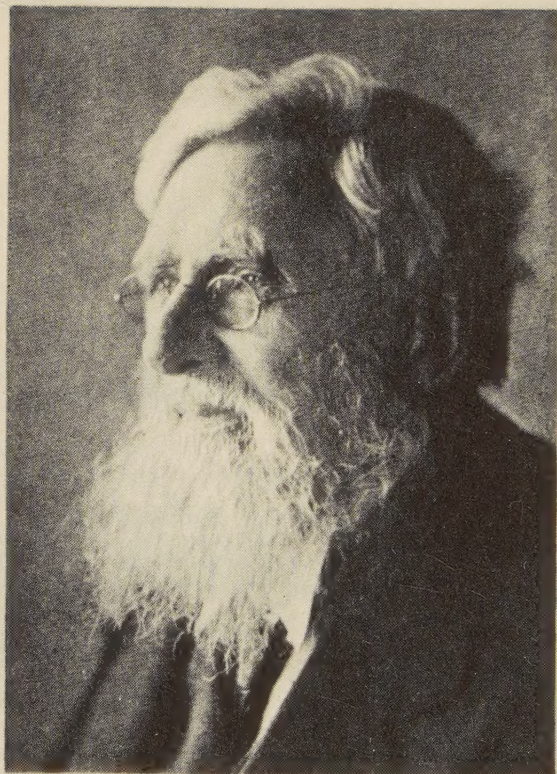


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Alfred R. Wallace

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LETTERS AND REMINISCENCES

BY
JAMES MARCHANT



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ALFRED RUSSEL WALLACE

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To
the Memory of
ANNIE WALLACE

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PREFACE

THIS volume consists of a selection from several thousands of letters entrusted to me by the Wallace family and dating from the dawn of Darwinism to the second decade of the twentieth century, supplemented by such biographical particulars and comments as are required for the elucidation of the correspondence and for giving movement and continuity to the whole.

The wealth and variety of Wallace's own correspondence, excluding the large collection of letters which he received from many eminent men and women, would be sufficient material to make four volumes. The family has given me unstinted confidence in using or rejecting letters and reminiscences, and although I have consulted scientific and literary friends, I alone must be blamed for sins of omission or commission. Nothing has been suppressed in the unpublished letters, or in any of the letters which appear in this volume, because there was anything to hide. Everything Wallace wrote, all his private letters, could be published to the world. His life was an open book—"no weakness, no contempt, dispraise, or blame, nothing but well and fair."

The profoundly interesting and now historic correspondence between Darwin and Wallace, part of which has already appeared in the "Life and Letters of Charles Darwin" and part in Wallace's autobiography, entitled "My Life," is here published, with new additions, for the first time as a whole, so that the reader now has before him the necessary material to form a true estimate of the origin and growth of the theory of Natural Selection, and of the personal relationships of its noble co-discoverers.

My warmest thanks are offered to Sir Francis Darwin for permission to use his father's letters, for his annotations, and for rendering help in checking the typescript of the Darwin letters; to Mr. John Murray, C.V.O., and to Messrs. D. Appleton & Co., for permission to use letters and notes from the "Life and Letters of Charles Darwin" and from "More Letters"; to Messrs. Chapman and Hall, and to Messrs. Dodd, Mead & Co., for their great generosity in allowing the free use of letters and material in Wallace's "My Life"; to Prof. E. B. Poulton, Prof. Sir W. F. Barrett, Sir Wm. Thiselton-Dyer, Dr. Henry Forbes, and others for letters and reminiscences; and to Prof. E. B. Poulton for reading the proofs and for valuable suggestions. An intimate chapter on Wallace's Home Life has been contributed by his son and daughter, Mr. W. G. Wallace and Miss Violet Wallace.

J. M.

MARCH, 1916.

ALFRED RUSSEL WALLACE
LETTERS AND REMINISCENCES

ALFRED RUSSEL WALLACE

LETTERS AND REMINISCENCES

INTRODUCTION

IN Westminster Abbey there repose, almost side by side, by no conscious design yet with deep significance, the mortal remains of Newton and Darwin. “‘The Origin of Species,’” said Wallace, “will live as long as the ‘Principia’ of Newton.” Near by are the tombs of Sir. J. Herschel, Lord Kelvin and Sir Charles Lyell; and the medallions in memory of Joule, Darwin, Stokes and Adams have been rearranged so as to admit similar memorials of Lister, Hooker and Alfred Russel Wallace. Now that the plan is completed, Darwin and Wallace are together in this wonderful galaxy of the great men of science of the nineteenth century. Several illustrious names are missing from this eminent company; foremost amongst them being that of Herbert Spencer, the lofty master of that synthetic philosophy which seemed to his disciples to have the proportions and qualities of an enduring monument, and whose incomparable fertility of creative thought entitled him to share the throne with Darwin. It was Spencer, Darwin, Wallace, Hooker, Lyell and Huxley who led that historic movement which garnered the work of Lamarck and Buffon, and gave new direction to the ceaseless interrogation of nature to discover the “how” and the “why” of the august progression of life.

Looking over the long list of the departed whose names are enshrined in our Minster, one has sorrowfully to observe that contemporary opinion of their place in history and abiding

worth was not infrequently astray; that memory has, indeed, forgotten their works; and their memorials might be removed to some cloister without loss of respect for the dead, perhaps even with the silent approval of their own day and generation could it awake from its endless sleep and review the strange and eventful course of human life since they left "this bank and shoal of time." But may it not be safely prophesied that of all the names on the starry scroll of national fame that of Charles Darwin will, surely, remain unquestioned? And entwined with his enduring memory, by right of worth and work, and we know with Darwin's fullest approval, our successors will discover the name of Alfred Russel Wallace. Darwin and Wallace were pre-eminent sons of light.

Among the great men of the Victorian age Wallace occupied a unique position. He was the co-discoverer of the illuminating theory of Natural Selection; he watched its struggle for recognition against prejudice, ignorance, ridicule and misrepresentation; its gradual adoption by its traditional enemies; and its final supremacy. And he lived beyond the hour of its signal triumph and witnessed the further advance into the same field of research of other patient investigators who are disclosing fresh phases of the same fundamental laws of development, and are accumulating a vast array of new facts which tell of still richer light to come to enlighten every man born into the world. To have lived through that brilliant period and into the second decade of the twentieth century; to have outlived all contemporaries, having been the co-revealer of the greatest and most far-reaching generalisation in an era which abounded in fruitful discoveries and in revolutionary advances in the application of science to life, is verily to have been the chosen of the gods.

Who and what manner of man was Alfred Russel Wallace? Who were his forbears? How did he obtain his insight into the closest secrets of nature? What was the extent of his contributions to our stock of human knowledge? In which directions did he most influence his age? What is known of his inner life? These are some of the questions which most present-day readers and all future readers into whose hands this book may come will ask.

As to his descent, his upbringing, his education and his esti-

mate of his own character and work, we can, with rare good fortune, refer them to this autobiography, in which he tells his own story and relates the circumstances which, combined with his natural disposition, led him to be a great naturalist and a courageous social reformer; nay, more, his autobiography is also in part a peculiar revelation of the inner man such as no biography could approach. We are also able to send inquirers to the biographies and works of his contemporaries—Darwin, Hooker, Lyell, Huxley and many others. All this material is already available to the diligent reader. But there are other sources of information which the present book discloses—Wallace's home-life, the large collection of his own letters, the reminiscences of friends, communications which he received from many co-workers and correspondents which, besides being of interest in themselves, often cast a sidelight upon his own mind and work. All these are of peculiar and intimate value to those who desire to form a complete estimate of Wallace. And it is to help the reader to achieve this desirable result that the present work is published.

It may be stated here that Wallace had suggested to the present writer that he should undertake a new work, to be called "Darwin and Wallace," which was to have been a comparative study of their literary and scientific writings, with an estimate of the present position of the theory of Natural Selection as an adequate explanation of the process of organic evolution. Wallace had promised to give as much assistance as possible in selecting the material without which the task on such a scale would obviously have been impossible. Alas! soon after the agreement with the publishers was signed and in the very month that the plan of the work was to have been shown to Wallace, his hand was suddenly stilled in death; and the book remains unwritten. But as the names of Darwin and Wallace are inseparable even by the scythe of time, a slight attempt is here made, in the first sections of Part I. and Part II., to take note of their ancestry and the diversities and similarities in their respective characters and environments—social and educational; to mark the chief characteristics of their literary works and the more salient conditions and events which led them, independently, to the idea of Natural Selection.

Finally, it may be remarked that up to the present time the

unique work and position of Wallace have not been fully disclosed owing to his great modesty and to the fact that he outlived all his contemporaries. "I am afraid," wrote Sir W. T. Thiselton-Dyer to him in one of his letters (1893), "the splendid modesty of the big men will be a rarer commodity in the future. No doubt many of the younger ones know an immense deal; but I doubt if many of them will ever exhibit the grasp of great principles which we owe to you and your splendid band of contemporaries." If this work helps to preserve the records of the influence and achievements of this illustrious and versatile genius and of the other eminent men who brought the great conception of evolution to light, it will surely have justified its existence.

PART I

I.—Wallace's Early Years

AS springs burst forth, now here, now there, on the mountain side, and find their way together to the vast ocean, so, at certain periods of history, men destined to become great are born within a few years of each other, and in the course of life meet and mingle their varied gifts of soul and intellect for the ultimate benefit of mankind. Between the years 1807 and 1825 at least eight illustrious scientists "saw the light"—Sir Charles Lyell, Sir Joseph Hooker, T. H. Huxley, Herbert Spencer, John Tyndall, Charles Darwin, Alfred Russel Wallace, and Louis Agassiz; whilst among statesmen and authors we recall Bismarck, Gladstone, Lincoln, Tennyson, Longfellow, Robert and Elizabeth Browning, Ruskin, John Stuart Blackie, and Oliver Wendell Holmes—a wonderful galaxy of shining names.

The first group is the one with which we are closely associated in this section in which we have brought together the names of Charles Darwin and Alfred Russel Wallace—between whose births there was a period of fourteen years, Darwin being born on the 12th of February, 1809, and Wallace on the 8th of January, 1823.

In each case we are indebted to an autobiography for an account of their early life and work written almost entirely from memory when at an age which enabled them to take an unbiased view of the past.

The autobiography of Darwin was written for the benefit of his family only, when he was 67; while the two large volumes entitled "My Life" were written by Wallace when he was 82 for the pleasure of reviewing his long career. These records are characterised by that charming modesty and simplicity of life and manner which was so marked a feature of both men.

In the circumstances surrounding their early days there was very little to indicate the similarity in character and mental gifts which became so evident in their later years. A brief outline of the hereditary influences immediately affecting them will enable us to trace something of the essential differences as well as the similarities which marked their scientific and literary attainments.

The earliest records of the Darwin family show that in 1500 an ancestor of that name (though spelt differently) was a substantial yeoman living on the borders of Lincolnshire and Yorkshire. In the reign of James I. the post of Yeoman of the Royal Armoury of Greenwich was granted to William Darwin, whose son served with the Royalist Army under Charles I. During the Commonwealth, however, he became a barrister of Lincoln's Inn, and later the Recorder of the City of Lincoln.

Passing over a generation, we find that a brother of Dr. Erasmus Darwin "cultivated botany," and, when far advanced in years, published a volume entitled "*Principia Botanica*," while Erasmus developed into a poet and philosopher. The eldest son of the latter "inherited a strong taste for various branches of science . . . and at a very early age collected specimens of all kinds." The youngest son, Robert Waring, father of Charles Darwin, became a successful physician, "a man of genial temperament, strong character, fond of society," and was the possessor of great psychic power by which he could readily sum up the characters of others and even occasionally read their thoughts. A judicious use of this gift was frequently found to be more efficacious than actual medicine! To the end of his life Charles Darwin entertained the greatest affection and reverence for his father, and frequently spoke of him to his own children.

From this brief summary of the family history it is easy to perceive the inherited traits which were combined in the attractive personality of the great scientist. From his early forbears came the keen love of sport and outdoor exercise (to which considerable reference is made in his youth and early manhood); the close application of the philosopher; and the natural aptitude for collecting specimens of all kinds. To his grandfather he was doubtless indebted for his poetic imagination, which, consciously or unconsciously, pervaded his thoughts and writ-

ings, saving them from the cold scientific atmosphere which often chills the lay mind. Lastly, the geniality of his father was strongly evidenced by his own love of social intercourse, his courtesy and ready wit, whilst the gentleness of his mother—who unfortunately died when he was seven years old—left a delicacy of feeling which pervaded his character to the very last.

No such sure mental influences, reaching back through several generations, can be traced in the records of the Wallace family, although what is known reveals the source of the dogged perseverance with which Wallace faced the immense difficulties met with by all early pioneer travellers, of that happy diversity of mental interests which helped to relieve his periods of loneliness and inactivity, and of that quiet determination to pursue to the utmost limit every idea which impressed his mind as containing the germ of a wider and more comprehensive truth than had yet been generally recognised and accepted.

The innate reticence and shyness of manner which were noticeable all through his life covered a large-heartedness even in the most careful observation of facts, and produced a tolerant disposition towards his fellow-men even when he most disagreed with their views or dogmas. He was one of those of whom it may be truly said in hackneyed phrases that he was “born great,” whilst destined to have “greatness thrust upon him” in the shape of honours which he received with hesitation.

From his autobiography we gather that his father, though dimly tracing his descent from the famous Wallace of Stirling, was born at Hanworth, in Middlesex, where there appears to have been a small colony of residents bearing the same name but occupying varied social positions, from admiral to hotel-keeper—the grandfather of Alfred Russel Wallace being known as a victualler. Thomas Vere Wallace was the only son of this worthy innkeeper; and, being possessed of somewhat wider ambitions than a country life offered, was articled to a solicitor in London, and eventually became an attorney-at-law. On his father's death he inherited a small private income, and, not being of an energetic disposition, he preferred to live quietly on it instead of continuing his practice. His main interests were somewhat literary and artistic, but without any definite aim; and this lack of natural energy, mental and physical, reappeared

in most of the nine children subsequently born to him, including Alfred Russel, who realised that had it not been for the one definite interest which gradually determined his course in life (an interest demanding steady perseverance and concentrated thought as well as physical enterprise), his career might easily have been much less useful.

It was undoubtedly from his father that he acquired an appreciation of good literature, as they were in the habit of hearing Shakespeare and similar works read aloud round the fireside on winter nights; whilst from his mother came artistic and business-like instincts—several of her relatives having been architects of no mean skill, combining with their art sound business qualities which placed them in positions of civic authority and brought them the respect due to men of upright character and good parts.

During the chequered experiences which followed the marriage of Thomas Vere Wallace and Mary Ann Greenell there appears to have been complete mutual affection and understanding. Although Wallace makes but slight reference to his mother's character and habits, one may readily conclude that her disposition and influence were such as to leave an indelible impression for good on the minds of her children, amongst her qualities being a talent for not merely accepting circumstances, but in a quiet way making the most of each experience as it came—a talent which we find repeated on many occasions in the life of her son Alfred.

It is a little curious that each of these great scientists should have been born in a house overlooking a well-known river—the home of the Darwins standing on the banks of the Severn, at Shrewsbury, and that of the Wallaces a stone's throw from the waters of the romantic and beautiful Usk, of Monmouthshire.

With remarkable clearness Dr. Wallace could recall events and scenes back to the time when he was only 4 years of age. His first childish experiment occurred about that time, due to his being greatly impressed by the story of the "Fox and the Pitcher" in *Æsop's Fables*. Finding a jar standing in the yard outside their house, he promptly proceeded to pour a small quantity of water into it, and then added a handful of small stones. The water not rising to the surface, as it did in the fable, he found a

spade and scraped up a mixture of earth and pebbles which he added to the stones already in the jar. The result, however, proving quite unsatisfactory, he gave it up in disgust and refused forthwith to believe in the truth of the fable. His restless brain and vivid imagination at this early period are shown by some dreams which he could still recall when 82 years of age; whilst the strong impression left on his mind by certain localities, with all their graphic detail of form and colour, enabled him to enjoy over again many of the simple pleasures which made up his early life in the beautiful grounds of the ancient castle in which he used to play.

The first great event in his life was the journey undertaken by ferry-boat and stage-coach from Usk to Hertford, to which town the family removed when he was 6 years old, and where they remained for the next eight years, until he left school.

The morning after their arrival an incident occurred which left its trace as of a slender golden thread running throughout the fabric of his long life. Alfred, with child-like curiosity about his new surroundings, wandered into the yard behind their house, and presently heard a voice coming from the other side of the low wall, saying, "Hallo! who are you?" and saw a boy about his own age peering over the top. Explanations followed, and soon, by the aid of two water-butts, the small boys found themselves sitting side by side on the top of the wall, holding a long and intimate conversation. Thus began his friendship with George Silk, and by some curious trend of circumstances the two families became neighbours on several subsequent occasions,¹ so that the friendship was maintained until in due course the boys separated each to his own way in life—the one to wander in foreign lands, the other to occupy a responsible position at home.

After spending about a year at private schools, Alfred Wallace was sent with his brother John to Hertford Grammar School. His recollections of these schooldays is full of interest, especially

¹ "While at Hertford I lived altogether in five different houses, and in three of these the Silk family lived next door to us, which involved not only each family having to move about the same time, but also that two houses adjoining each other should have been vacant together, and that they should have been of the size required by each, which after the first was not the same, the Silk family being much the larger."—"My Life," i. 32.

as contrasted with the school life of to-day. He says: "We went to school even in the winter at seven in the morning, and three days a week remained till five in the afternoon; some artificial light was necessary, and this was effected by the primitive method of every boy bringing his own candle or candle-ends with any kind of candlestick he liked. An empty ink-bottle was often used, or the candle was even stuck on to the desk with a little of its own grease. So that it enabled us to learn our lessons or do our sums, no one seemed to trouble about how we provided the light."

Though never robust in health, he enjoyed all the usual boyish sports, especially such as appealed to his imagination and love of adventure. Not far from the school a natural cave, formed in a chalky slope and partially concealed by undergrowth, made an excellent resort for "brigands"; and to this hiding-place were brought potatoes and other provisions which could be cooked and eaten in primitive fashion, with an air of secrecy which added to the mystery and attraction of the boyish adventure.

It is curious to note that one destined to become a great traveller and explorer should have found the study of geography "a painful subject." But this was, as he afterwards understood, entirely due to the method of teaching then, and sometimes now in vogue, which made no appeal whatever to the imagination by creating a mental picture of the peoples and nations, or the varied wonders and beauties of nature which distinguish one country from another. "No interesting facts were ever given, no accounts of the country by travellers were ever read, no good maps ever given us, nothing but the horrid stream of unintelligible place-names to be learnt." The only subjects in which he considered that he gained some valuable grounding at school were Latin, arithmetic, and writing.

This estimate of the value of the grammar-school teaching is echoed in Darwin's own words when describing his schooldays at precisely the same age at Shrewsbury Grammar School, where, he says, "the school as a means of education to me was simply a blank." It is therefore interesting to notice, side by side, as it were, the occupation which each boy found for himself out of school hours, and which in both instances proved of immense value in their respective careers in later life.

Darwin, even at this early age, found his "taste for natural

history, and more especially for collecting," well developed. "I tried," he says, "to make out the names of plants, and collected all sorts of things, shells, seals, franks, coins and minerals. The passion for collecting which leads a man to be a systematic naturalist . . . was very strong in me, and was clearly innate, as none of my sisters or brothers ever had this taste."

He also speaks of himself as having been a very "simple little fellow" by the manner in which he was either himself deceived or tried to deceive others in a harmless way. As an instance of this, he remembered declaring that he could "produce variously coloured polyanthuses and primroses by watering them with certain coloured fluids," though he knew all the time it was untrue. His feeling of tenderness towards all animals and insects is revealed in the fact that he could not remember—except on one occasion—ever taking more than one egg out of a bird's nest; and though a keen angler, as soon as he heard that he could kill the worms with salt and water he never afterwards "spitted a living worm, though at the expense, probably, of some loss of success!"

Nothing thwarted young Darwin's intense joy and interest in collecting minerals and insects, and in watching and making notes upon the habits of birds. In addition to this wholesome outdoor hobby, the tedium of school lessons was relieved for him by reading Shakespeare, Byron and Scott—also a copy of "Wonders of the World" which belonged to one of the boys, and to which he always attributed his first desire to travel in remote countries, little thinking how his dreams would be fulfilled.

Whilst Charles Darwin occupied himself with outdoor sport and collecting, with a very moderate amount of reading thrown in at intervals, Wallace, on the contrary, devoured all the books he could get; and fortunately for him, his father having been appointed Librarian to the Hertford Town Library, Alfred had access to all the books that appealed to his mental appetite; and these, especially the historical novels, supplemented the lack of interesting history lessons at school, besides giving him an insight into many kinds of literature suited to his varied tastes and temperament. In addition, however, to the hours spent in reading, he and his brother John found endless delight in turning the loft of an outhouse adjoining their yard into a sort of mechanical factory. Here they contrived, by saving up all

their pence (the only pocket-money that came to them), to make crackers and other simple fireworks, and to turn old keys into toy cannon, besides making a large variety of articles for practical domestic purposes. Thus he cultivated the gift of resourcefulness and self-reliance upon which he had so often to depend when far removed from all civilisation during his travels on the Amazon and in the Malay Archipelago.

A somewhat amusing instance of this is found in a letter to his sister, dated June 25th, 1855, at a time when he wanted a really capable man for his companion, in place of his good-natured but incapable "boy" Charles, whom he had brought with him from London to teach collecting. In reply to some remarks by his sister about a young man who she thought would be suitable, he wrote: "Do not tell me merely that he is 'a very nice young man.' Of course he is. . . . I should like to know whether he can live on rice and salt fish for a week on occasion. . . . Can he sleep on a board? . . . Can he walk twenty miles a day? Whether he can work, for there is sometimes as hard work in collecting as in anything. Can he saw a piece of wood straight? Ask him to make you anything—a little card box, a wooden peg or bottle-stopper, and see if he makes them neat and square."

In another letter he describes the garden and live stock he had been able to obtain where he was living; and in yet another he gives a long list of his domestic woes and tribulation—which, however, were overcome with the patience inculcated in early life by his hobbies, and also by the fact that the family was always more or less in straitened circumstances, so that the children were taught to make themselves useful in various ways in order to assist their mother in the home.

As he grew from childhood into youth, Alfred Wallace's extreme sensitiveness developed to an almost painful degree. He grew rapidly, and his unusual height made him still more shy when forced to occupy any prominent position amongst boys of his own age. During the latter part of his time at Hertford Grammar School his father was unable to pay the usual fees, and it was agreed that Alfred should act as pupil teacher in return for the lessons received. This arrangement, while acceptable on the one hand, caused him actual mental and physical pain on the other, as it increased his consciousness of the dis-

abilities under which he laboured in contrast to most of the other boys of his own age.

At the age of 14 Wallace was taken away from school, and until something could be definitely decided about his future—as up to the present he had no particular bent in any one direction—he was sent to London to live with his brother John, who was then working for a master-builder in the vicinity of Tottenham Court Road. This was in January, 1837, and it was during the following summer that he joined his other brother, William, at Barton-on-the-Clay, Bedfordshire, and began land surveying. In the meantime, while in London, he had been brought very closely into contact with the economics and ethics of Robert Owen, the well-known Socialist; and though very young in years he was so deeply impressed with the reasonableness and practical outcome of these theories that, though considerably modified as time went on, they formed the foundation for his own writings on Socialism and allied subjects in after years.

As one of our aims in this section is to suggest an outline of the contrasting influences governing the early life of Wallace and Darwin, it is interesting to note that at the ages of 14 and 16, respectively, and immediately on leaving school, they came under the first definite mental influence which was to shape their future thought and action. Yet how totally different from Wallace's trials as a pupil teacher was the removal of Darwin from Dr. Butler's school at Shrewsbury because "he was doing no good" there, and his father thought it was "time he settled down to his medical study in Edinburgh," never heeding the fact that his son had already one passion in life, apart from "shooting, dogs and rat-catching," which stood a very good chance of saving him from becoming the disgrace to the family that his good father feared. So that while Wallace, at 14 years of age, was imbibing his first lessons in Socialism, Darwin at 16 found himself merely enduring, with a feeling of disgust, Dr. Duncan's lectures, which were "something fearful to remember," on *materia medica* at eight o'clock on a winter's morning, and, worse still, Dr. Munro's lectures on human anatomy, which were "as dull as he was himself." Yet he always deeply regretted not having been urged to practise dissection, because of the invaluable aid it would have been to him as a naturalist.

By mental instinct, however, Darwin soon found himself

studying marine zoology and other branches of natural science. This was in a large measure due to his intimacy with Dr. Grant, who, in a later article on *Flustra*, made some allusion to a paper read by Darwin before the Linnean Society on a small discovery which he had made by the aid of a "wretched microscope" to the effect that the so-called ova of *Flustra* were really larvæ and had the power of independent action by the means of cilia.

During his second year in Edinburgh he attended Jameson's lectures on geology and zoology, but found them so "incredibly dull" that he determined never to study the science.

Then came the final move which, all unknowingly, was to lead Darwin into the pursuit of a science which up to that time had only been a hobby and not in any sense the serious profession of his life. But again how wide the difference between his change from Edinburgh to Cambridge, and that of Wallace from a month's association with a working-class Socialistic community in London to land surveying under the simplest rural conditions prevalent amongst the respectable labouring farmers of Bedfordshire—Darwin to the culture and privileges of a great University with the object of becoming a clergyman, and Wallace taking the first road that offered towards earning a living, with no thought as to the ultimate outcome of this life in the open and the systematic observation of soils and land formation.

But the inherent tendencies of Darwin's nature drew him away from theology to the study of geology, entomology and botany. The ensuing four years at Cambridge were very happy ones. While fortunate in being able to follow his various mental and scientific pursuits with the freedom which a good social and financial position secured for him, he found himself by a natural seriousness of manner, balanced by a cheerful temperament and love of sport, the friend and companion of men many years his seniors and holding positions of authority in the world of science. Amongst these the name of Professor Henslow will always take precedence. "This friendship," says Darwin, "influenced my whole career more than any other." Henslow's extensive knowledge of botany, geology, entomology, chemistry and mineralogy, added to his sincere and attractive personality, well-balanced mind and excellent judgment, formed a strong and effective bias in the direction Darwin was destined to follow.

Apart, however, from the strong personal influence of Hen-

slow, Sedgwick and others with whom he came much in contact, two books which he read at this time aroused his "burning zeal to add the most humble contribution to the noble structure of Natural Science"; these were Sir J. Herschel's "Introduction to the Study of Natural Philosophy," and Humboldt's "Personal Narrative." Indeed, so fascinated was he with the description given of Teneriffe in the latter that he at once set about a plan whereby he might spend a holiday, with Henslow, in that locality, a holiday which was, indeed, to form part of his famous voyage.

By means of his explorations in the neighbourhood of Cambridge, and one or two visits to North Wales, Darwin's experimental knowledge of geology and allied sciences was considerably increased. In his zeal for collecting beetles he employed a labourer to "scrape the moss off old trees in winter, and place it in a bag, and likewise to collect the rubbish at the bottom of the barges in which reeds were brought from the fens, and thus . . . got some very rare species."

During the summer vacation of 1831, at the personal request of Henslow, he accompanied Professor Sedgwick on a geological tour in North Wales. In order, no doubt, to give him some independent experience, Sedgwick sent Darwin on a line parallel with his own, telling him to bring back specimens of the rocks and to mark the stratification on a map. In later years Darwin was amazed to find how much both of them had failed to observe, "yet these phenomena were so conspicuous that . . . a house burnt down by fire could not tell its story more plainly than did the valley of Cwm Idwal."

This tour was the introduction to a momentous change in his life. On returning to Shrewsbury he found a letter awaiting him which contained the offer of a voyage in H.M.S. *Beagle*. But owing to several objections raised by Dr. Darwin, he wrote and declined the offer; and if it had not been for the immediate intervention of his uncle, Mr. Josiah Wedgwood (to whose house he went the following day to begin the shooting season), who took quite a different view of the proposition, the "Journal of Researches during the Voyage of H.M.S. *Beagle*," by Charles Darwin, would never have been written.

At length, however, after much preparation and many delays, the *Beagle* sailed from Plymouth on December 27th, 1831, and five years elapsed before Darwin set foot again on English soil,

The period, therefore, in Darwin's life which we find covered by his term at Edinburgh and Cambridge, until at the age of 22 he found himself suddenly launched on an entirely new experience full of adventure and fresh association, was spent by Wallace in a somewhat similar manner in so far as his outward objective in life was more or less distinct from the pursuits which gradually dawned upon his horizon, though they were followed as a "thing apart" and not as an ultimate end.

With Wallace's removal into Bedfordshire an entirely new life opened up before him. His health, never very good, rapidly improved; both brain and eye were trained to practical observations which proved eminently valuable. His descriptions of people with whom he came in contact during these years of country life reveal the quiet toleration of the faults and foibles of others, not devoid of the keen sense of humour and justice which characterised his life-long attitude towards his fellow-men.

The many interests of his new life, together with the use of a pocket sextant, prompted him to make various experiments for himself. The only sources from which he could obtain helpful information, however, were some cheap elementary books on mechanics and optics which he procured from the Society for the Diffusion of Useful Knowledge; these he studied and "puzzled over" for several years. "Having no friends of my own age," he wrote, "I occupied myself with various pursuits in which I had begun to take an interest. Having learnt the use of the sextant in surveying, and my brother having a book on Nautical Astronomy, I practised a few of the simpler observations. Among these were determining the meridian by equal altitudes of the sun, and also by the pole star at its upper or lower culmination; finding the latitude by the meridian altitude of the sun, or of some of the principal stars; and making a rude sundial by erecting a gnomon towards the pole. For these simple calculations I had Hannay and Dietrichsen's Almanac, a copious publication which gave all the important data in the Nautical Almanac, besides much other interesting matter useful for the astronomical amateur or the ordinary navigator. I also tried to make a telescope by purchasing a lens of about 2 ft. focus at an optician's in Swansea, fixing it in a paper tube and using the eye-piece of a small opera-glass. With it I was able

to observe the moon and Jupiter's satellites, and some of the larger star-clusters; but, of course, very imperfectly. Yet it served to increase my interest in astronomy, and to induce me to study with some care the various methods of construction of the more important astronomical instruments; and it also led me throughout my life to be deeply interested in the grand onward march of astronomical discovery."¹

At the same time Wallace became attracted by, and interested in, the flowers, shrubs and trees growing in that part of Bedfordshire, and he acquired some elementary knowledge of zoology. 'It was,' he writes, "while living at Barton that I obtained my first information that there was such a science as geology. . . . My brother, like most land-surveyors, was something of a geologist, and he showed me the fossil oysters of the genus *Gryphæa* and the *Belemnites* . . . and several other fossils which were abundant in the chalk and gravel around Barton. . . . It was here, too, that during my solitary rambles I first began to feel the influence of nature and to wish to know more of the various flowers, shrubs and trees I daily met with, but of which for the most part I did not even know the English names. At that time I hardly realised that there was such a science as systematic botany, that every flower and every meanest and most insignificant weed had been accurately described and classified, and that there was any kind of system or order in the endless variety of plants and animals which I knew existed. This wish to know the names of wild plants, to be able to speak . . . about them, had arisen from a chance remark I had overheard about a year before. A lady . . . whom we knew at Hertford, was talking to some friends in the street when I and my father met them . . . (and) I heard the lady say, 'We found quite a rarity the other day—the *Monotropa*; it had not been found here before.' This I pondered over, and wondered what the *Monotropa* was. All my father could tell me was that it was a rare plant; and I thought how nice it must be to know the names of rare plants when you found them."²

One can picture the tall quiet boy going on these solitary rambles, his eye becoming gradually quickened to perceive new forms in nature, contrasting one with the other, and beginning

¹ "My Life," i. 191-2.

² "My Life," i. 108-111.

to ponder over the *cause* which led to the diverse formation and colouring of leaves apparently of the same family.

It was in 1841, four years later, that he heard of, and at once procured, a book published at a shilling by the S.P.C.K. (the title of which he could not recall in after years), to which he owed his first scientific glimmerings of the vast study of Botany. The next step was to procure, at much self-sacrifice, Lindley's "Elements of Botany," published at half a guinea, which to his immense disappointment he found of very little use, as it did not deal with British plants! His disappointment was lessened, however, by the loan from a Mr. Hayward of Loudon's "Encyclopædia of Plants," and it was with the help of these two books that he made his first classification of the specimens which he had collected and carefully kept during the few preceding years.

"It must be remembered," he says in "My Life," "that my ignorance of plants at this time was extreme. I knew the wild rose, bramble, hawthorn, buttercup, poppy, daisy and foxglove, and a very few others equally common. . . . I knew nothing whatever as to genera and species, nor of the large number of distinct forms related to each and grouped into natural orders. My delight, therefore, was great when I was . . . able to identify the charming little eyebright, the strange-looking cow-wheat and louse-wort, the handsome mullein and the pretty creeping toad-flax, and to find that all of them, as well as the lordly foxglove, formed parts of one great natural Order, and that under all their superficial diversity of form was a similarity of structure which, when once clearly understood, enabled me to locate each fresh species with greater ease." This, however, was not sufficient, and the last step was to form a herbarium.

"I soon found," he wrote, "that by merely identifying the plants I found in my walks I lost much time in gathering the same species several times, and even then not being always quite sure that I had found the same plant before. I therefore began to form a herbarium, collecting good specimens and drying them carefully between drying papers and a couple of boards weighted with books or stones. . . . I first named the species as nearly as I could do so, and then laid them out to be pressed and dried. At such times," he continues—and I have quoted the passage for the sake of this revealing confession—"I experienced

the joy which every discovery of a new form of life gives to the lover of nature, almost equal to those raptures which I afterwards felt at every capture of new butterflies on the Amazon, or at the constant stream of new species of birds, beetles and butterflies in Borneo, the Moluccas, and the Aru Islands." ¹

Anything in the shape of gardening papers and catalogues which came in his way was eagerly read, and to this source he owed his first interest in the fascinating orchid.

"A catalogue published by a great nurseryman in Bristol . . . contained a number of tropical orchids, of whose wonderful variety and beauty I had obtained some idea from the woodcuts in Loudon's Encyclopædia. The first epiphytal orchid I ever saw was at a flower show in Swansea . . . which caused in me a thrill of enjoyment which no other plant in the show produced. My interest in this wonderful order of plants was further enhanced by reading in the *Gardener's Chronicle* an article by Dr. Lindley on one of the London flower shows, where there was a good display of orchids, in which . . . he added, 'and *Dendrobium Devonianum*, too delicate and beautiful for a flower of earth.' This and other references . . . gave them, in my mind, a weird and mysterious charm . . . which, I believe, had its share in producing that longing for the tropics which a few years later was satisfied in the equatorial forests of the Amazon." ²

For a brief period, when there was a lull in the surveying business and his prospects of continuing in this profession looked uncertain, he tried watchmaking, and would probably—though not by choice—have been apprenticed to it but for an unexpected circumstance which caused his master to give up his business. Alfred gladly, when the occasion offered, returned to his outdoor life, which had begun to make the strongest appeal to him, stronger, perhaps, than he was really aware.

Early in 1844 another break occurred, due to the sudden falling off of land surveying as a profitable business. His brother could no longer afford to keep him as assistant, finding

¹ Darwin makes a similar comment: "I was very successful in collecting, and invented two new methods . . . and thus I got some very rare species. No poet ever felt more delighted at seeing his first poem published than I did at seeing, in Stephens' 'Illustrations of British Insects,' the magic words, captured by C. Darwin, Esq.'"—"Darwin's Autobiography," in the one-volume "Life," p. 20.

² "My Life," i. 194-5.

it indeed difficult to obtain sufficient employment for himself. As Wallace knew no other trade or profession, the only course which occurred to his mind as possible by which to earn a living was to get a post as school teacher.

After one or two rather amusing experiences, he eventually found himself in very congenial surroundings under the Rev. Abraham Hill, headmaster of the Collegiate School at Leicester. Here he stayed for a little more than a year, during which time—in addition to his school work and a considerable amount of hard reading on subjects to which he had not hitherto been able to devote himself—he was led to become greatly interested in phrenology and mesmerism, and before long found himself something of an expert in giving mesmeric demonstrations before small audiences. Phrenology, he believed, proved of much value in determining his own characteristics, good and bad, and in guiding him to a wise use of the faculties which made for his ultimate success; while his introduction to mesmerism had not a little to do with his becoming interested and finally convinced of the part played by spiritualistic forces and agencies in human life.

The most important event, however, during this year at Leicester was his meeting with H. W. Bates, through whom he was introduced to the absorbing study of beetles and butterflies, the link which culminated in their mutual exploration of the Amazon. It is curious that Wallace retained no distinct recollection of how or when he met Bates for the first time, but thought that "he heard him mentioned as an enthusiastic entomologist and met him at the Library." Bates was at this time employed by his father, who was a hosiery manufacturer, and he could therefore only devote his spare time to collecting beetles in the surrounding neighbourhood. The friendship brought new interests into both lives, and though Wallace was obliged a few months later to leave Leicester and return to his old work of surveying (owing to the sudden death of his brother William, whose business affairs were left in an unsatisfactory condition and needed personal attention), he no longer found in it the satisfaction he had previously experienced, and his letters to Bates expressed the desire to strike out on some new line, one which would satisfy his craving for a definite pursuit in the direction of natural science.

Somewhere about the autumn of 1847, Bates paid a visit to

Wallace at Neath, and the plan to go to the Amazon which had been slowly forming itself at length took shape, due to the perusal of a little book entitled "A Voyage up the River Amazon," by W. H. Edwards. Further investigations showed that this would be particularly advantageous, as the district had only been explored by the German zoologist von Spix, and the botanist von Martins, in 1817-20, and subsequently by Count de Castelnau.

During this interval we find, in a letter to Bates, the following allusion to Darwin, which is the first record of Wallace's high estimate of the man with whom his own name was to be dramatically associated ten years later. "I first," he says, "read Darwin's Journal three or four years ago, and have lately re-read it. As the journal of a scientific traveller it is second only to Humboldt's Narrative; as a work of general interest, perhaps superior to it. He is an ardent admirer and most able supporter of Mr. Lyell's views. His style of writing I very much admire, so free from all labour, or egotism, yet so full of interest and original thought."¹

The early part of 1848 was occupied in making arrangements with Mr. Samuel Stevens, of King Street, Covent Garden, to act as their agent in disposing of a duplicate collection of specimens which they proposed sending home; by this means paying their expenses during the time they were away, any surplus being invested against their return. This and other matters being satisfactorily settled, they eventually sailed from Liverpool on April 20th in a barque of 192 tons, said to be "a very fast sailer," which proved to be correct. On arriving at Para about a month later, they immediately set about finding a house, learning something of the language, the habits of the people amongst whom they had come to live, and in making short excursions into the forest before starting on longer and more trying explorations up country.

Wallace's previous vivid imaginings of what life in the tropics would mean, so far as the surpassing beauty of nature was concerned, were not immediately fulfilled. As a starting-point, however, Para had many advantages. Besides the pleasant

¹ There is no record in his autobiography as to the exact date when he first became acquainted with Lyell's work, though several times reference is made to it.

climate, the country for some hundreds of miles was found to be nearly level at an elevation of about 30 or 40 ft. above the river; the first distinct rise occurring some 150 miles up the river Tocantins, southwest of Para; the whole district was intersected by streams, with cross channels connecting them, access by this means being comparatively easy to villages and estates lying farther inland.

Before making an extensive excursion into the interior, he spent some time on the larger islands at the mouth of the Amazon, on one of which he immediately noticed the scarcity of trees, while "the abundance of every kind of animal life crowded into a small space was here very striking, compared with the sparse manner in which it is scattered in the virgin forests. It seems to force us to the conclusion that the luxuriance of tropical vegetation is not favourable to the production of animal life. The plains are always more thickly peopled than the forest; and a temperate zone, as has been pointed out by Mr. Darwin, seems better adapted to the support of large land animals than the tropics."

We have already referred to the fact that at the very early age of 14 Wallace had imbibed his first ideas of Socialism, or how the "commonwealth" of a people or nation was the outcome of cause and effect, largely due to the form of government, political economy and progressive commerce best suited to any individual State or country. The seed took deep root, and during the years spent for the most part amongst an agricultural people in England and Wales his interest in these questions had been quickened by observation and intelligent inquiry. It is no wonder, therefore, that during the whole of his travels we find many intimate references to such matters regarding the locality in which he happened to find himself, but which can only be noticed in a very casual manner in this section. For instance, he soon discovered that the climate and soil round Para conduced to the cultivation of almost every kind of food, such as cocoa, coffee, sugar, farinha (the universal bread of the country) from the mandioca plant, with vegetables and fruits in inexhaustible variety; while the articles of export included india-rubber, Brazil nuts, and piassaba (the coarse, stiff fibre of a palm, used for making brooms for street sweepings), as well as sarsaparilla, balsam capivi, and a few other drugs.

The utter lack of initiative, or even ordinary interest, in making the most of the opportunities lying at hand, struck him again and again as he went from place to place and was entertained hospitably by hosts of various nationalities; until at times the impression is conveyed that apart from his initial interest as a naturalist, a longing seized him to arouse those who were primarily responsible for these conditions out of the apathy into which they had fallen, and to make them realise the larger pleasure which life offers to those who recognise the opportunities at hand, not only for their own advancement but also for the benefit of those placed under their control. All of which we find happily illustrated during his visit to Sarawak, in the Malay Archipelago.

The whole of these four years was crowded with valuable experiences of one sort and another. Some of the most toilsome journeys proved only a disappointment, while others brought success beyond his most sanguine dreams. At the end of two years it was agreed between himself and Bates that they should separate, Wallace doing the northern parts and tributaries of the Amazon, and Bates the main stream, which, from the fork of the Rio Negro, is called the Upper Amazon, or the Solimoes. By this arrangement they were able to cover more ground, besides devoting themselves to the special goal of research on which each was bent.

I the meantime, Wallace's younger brother, Herbert, had come out to join him, and for some time their journeys were made conjointly; but finding that his brother was not temperamentally fitted to become a naturalist, it was decided that he should return to England. Accordingly, they parted at Barra when Wallace started on his long journey up the Rio Negro, the duration of which was uncertain; and it was not until many months after the sad event that he heard the distressing news that Herbert had died of yellow fever on the eve of his departure from Para for home. Fortunately, Bates was in Para at the time, and did what he could for the boy until stricken down himself with the same sickness, from which, however, his stronger constitution enabled him to recover.

Perhaps the most eventful and memorable journey during this period was the exploration of the Uaupes River, of which Wallace wrote nearly sixty years later: "So far as I have heard,

no English traveller has to this day ascended the Uaupes River so far as I did, and no collector has stayed at any time at Javita, or has even passed through it."

From a communication received from the Royal Geographical Society it appears that the first complete survey of this river (a compass traverse supplemented by astronomical observations) was made (1907-8) by Dr. Hamilton Rice, starting from the side of Colombia, and tracing the whole course of the river from a point near the source of its head-stream. The result showed that the general course of the lower river was much as represented by Wallace, though considerable corrections were necessary both in latitude and longitude. "I am assured by authorities on the Rio Negro region," writes Dr. Scott Keltie to Mr. W. G. Wallace, under date May 21, 1915, "that your father's work still holds good."

In May, 1852, Wallace returned to Para, and sailed for England the following July. The ship took fire at sea, and all his treasures (not previously sent to England) were unhappily lost. Ten days and nights were spent in an open boat before another vessel picked them up, and in describing this terrible experience he says: "When the danger appeared past I began to feel the greatness of my loss. With what pleasure had I looked upon every rare and curious insect I had added to my collection! How many times, when almost overcome by the ague, had I crawled into the forest and been rewarded by some unknown and beautiful species! How many places, which no European foot but my own had trodden, would have been recalled to my memory by the rare birds and insects they had furnished to my collection! How many weary days and weeks had I passed, upheld only by the fond hope of bringing home many new and beautiful forms from these wild regions . . . which would prove that I had not wasted the advantage I had enjoyed, and would give me occupation and amusement for many years to come! And now . . . I had not one specimen to illustrate the unknown lands I had trod, or to call back the recollection of the wild scenes I had beheld! But such regrets were vain . . . and I tried to occupy myself with the state of things which actually existed."¹

On reaching London, Wallace took a house in Upper Albany

¹ "Travels on the Amazon," 277.

Street, where his mother and his married sister (Mrs. Sims), with her husband, a photographer, came to live with him. The next eighteen months were fully occupied with sorting and arranging such collections as had previously reached England; writing his book of travels up the Amazon and Rio Negro (published in the autumn of 1853), and a little book on the palm trees based on a number of fine pencil sketches he had preserved in a tin box, the only thing saved from the wreck.

In summing up the most vivid impressions left on his mind, apart from purely scientific results, after his four years in South America, he wrote that the feature which he could never think of without delight was "the wonderful variety and exquisite beauty of the butterflies and birds . . . ever new and beautiful, strange and even mysterious," so that he could "hardly recall them without a thrill of admiration and wonder." But "the most unexpected sensation of surprise and delight was my first meeting and living with man in a state of nature—with absolute uncontaminated savages! . . . and the surprise of it was that I did not expect to be at all so surprised. . . . These true wild Indians of the Uaupes . . . had nothing that we call clothes; they had peculiar ornaments, tribal marks, etc.; they all carried tools or weapons of their own manufacture. . . . But more than all, their whole aspect and manner was different—they were all going about their own work or pleasure, which had nothing to do with white men or their ways; they walked with the free step of the independent forest-dweller, and, except the few that were known to my companion, paid no attention whatever to us, mere strangers of an alien race! In every detail they were original and self-sustaining as are the wild animals of the forest, absolutely independent of civilisation. . . . I could not have believed that there would have been so much difference in the aspect of the same people in their native state and when living under European supervision. The true denizen of the Amazonian forest, like the forest itself, is unique and not to be forgotten."

The foregoing "impressions" recall forcibly those expressed by Darwin in similar terms at the close of his "Journal": "Delight . . . is a weak term to express the feelings of a naturalist who, for the first time, has wandered by himself in a Brazilian forest. The elegance of the grasses, the novelty of the parasitical plants, the beauty of the flowers, the glossy green of the foliage

. . . the general luxuriance of the vegetation, filled me with admiration. A paradoxical mixture of sound and silence pervades the shady parts of the wood . . . yet within the recesses . . . a universal silence appears to reign . . . such a day as this brings with it a deeper pleasure than he (a naturalist) can ever hope to experience again."¹ And in another place: "Among the scenes which are deeply impressed on my mind, none can exceed in sublimity the primeval forests undefaced by the hand of man; . . . temples filled with the various productions of the God of Nature; . . . no one can stand in these solitudes unmoved, and not feel that there is more in man than the mere breath of his body."²

In complete contrast to the forest, the bare, treeless and uninhabited plains of Patagonia "frequently crossed before" Darwin's eyes. Why, he could not understand, except that, being so "boundless," they left "free scope for the imagination."

As these travels,³ undertaken at comparatively the same age, represent the foundation upon which their scientific work and theories were based during the long years which followed, a glance at the conditions governing the separate expeditions—both mental and physical—may be of some value. The most obvious difference lies, perhaps, in the fact that Darwin was free from the thought of having to "pay his way" by the immediate result of his efforts, and likewise from all care and anxiety regarding domestic concerns; the latter being provided for him when on board the *Beagle*, or arranged by those who accompanied him on his travels overland and by river. The elimination of these minor cares tended to leave his mind free and open to absorb and speculate at comparative leisure upon all the strange phenomena which presented themselves throughout the long voyage.

A further point of interest in determining the ultimate gain or loss lies in the fact that Darwin's private excursions had to be somewhat subservient to the movements of the *Beagle* under the command of Captain Fitz-Roy. This, in all probability, was beneficial to one of his temperament—unaccustomed to be

¹ "Voyage of the *Beagle*," pp. 11-12.

² *Ibid.*, p. 534.

³ It is interesting to note that the careers of Sir Joseph Hooker, Charles Darwin, H. W. Bates, Alfred Russel Wallace, and T. H. Huxley were all determined by voyages or journeyings of exploration.

greatly restricted by outward circumstances or conditions, though never flagrantly (or, perhaps, consciously) going against them. The same applies in a measure to Wallace, who, on more than one occasion, confessed his tendency to a feeling of semi-idleness and dislike to any form of enforced physical exertion; but as every detail, involving constant forethought and arrangement, as well as the execution, devolved upon himself, the latent powers of methodical perseverance, which never failed him, no matter what difficulties barred his way, were called forth. Darwin's estimate of the "habit of mind" forced upon himself during this period may not inaptly be applied to both men: "Everything about which I thought or read was made to bear directly on what I had seen, or was likely to see; and this habit of mind was continued during the five years of the voyage. I feel sure that it was this training which enabled me to do whatever I have done in science."

It may be further assumed that Darwin was better equipped mentally—from a scientific point of view—owing to his personal intercourse with eminent scientific men previous to his assuming this responsible position. Wallace, on the contrary, had practically little beyond book-knowledge and such experience as he had been able to gain by solitary wanderings in the localities in which he had, by circumstances, been forced to reside. His plan of operations must, therefore, have been largely modified and adapted as time went on, and as his finances allowed. To both, therefore, credit is due for the adaptability evinced under conditions not always congenial or conducive to the pursuits they had undertaken.

Although the fact is not definitely stated by Wallace, it may readily be inferred that the idea of making this the starting-point of a new life was clearly in his mind; while Darwin simply accepted the opportunity when it came, and was only brought to a consciousness of its full meaning and bearing on his future career whilst studying the geological aspect of Santiago when "the line of white rock revealed a new and important fact," namely, that there had been afterwards subsidence round the craters, which had since been in action and had poured forth lava. "It then," he says, "first dawned on me that I might perhaps write a book on the geology of the various countries visited, and this made me thrill with delight. That was a mem-

orable hour to me; and how distinctly I can call to mind the low cliff of lava, beneath which I rested, with the sun glaring hot, a few strange desert plants growing near, and with living corals in the tidal pools at my feet!"¹

Another point of comparison lies in the fact that at no time did the study of man or human nature, from the metaphysical and psychological point of view, appeal to Darwin as it did to Wallace; and this being so, the similarity between the impression made on them individually by their first contact with primitive human beings is of some interest.

Wallace's words have already been quoted; here are Darwin's: "Nothing is more certain to create astonishment than the first sight in his native haunt of a barbarian, of man in his lowest and most savage state. One asks: 'Could our progenitors have been men like these—men whose very signs and expressions are less intelligible to us than those of the domesticated animals; men who do not possess the instinct of those animals, nor yet appear to boast of human reason, or at least of arts consequent on that reason?' I do not believe it is possible to describe or paint the difference between a savage and civilised man. It is the difference between a wild and tame animal."²

The last words suggest the seed-thought eventually to be enlarged in his "The Descent of Man," and there is also perhaps a subtle suggestion of the points in which Wallace differed from Darwin when the time came for them to discuss this important section of the theory of evolution. It needed, however, the further eight years spent by Wallace in the Malay Archipelago to bring about a much wider knowledge of nature-science before he was prepared in any way to assume the position of exponent of theories not seriously thought of previously in the scientific world.

In the autumn of 1853, on the completion of his "Travels on the Amazon and Rio Negro," Wallace paid his first visit to Switzerland, on a walking tour in company with his friend George Silk. On his return, and during the winter months, he was constant in his attendance at the meetings of the Entomological and Zoological Societies. It was at one of these evening gather-

¹ "Life of Charles Darwin" (one-volume edit.), p. 20.

² "Voyage of the *Beagle*," p. 535.

ings that he first met Huxley, and he also had a vague recollection of once meeting and speaking to Darwin at the British Museum. Had it not been for his extreme shyness of disposition, and (according to his own estimation) "lack of conversational powers," he would doubtless have become far more widely known, and have enjoyed the friendship of not a few of the eminent men who shared his interests, during this interval before starting on his journey to Singapore.

It was due to his close study of the Insect and Bird Departments of the British Museum that he decided on Singapore as a new starting-point for his natural history collections. As the region was generally healthy, and no part of it (with the exception of the Island of Java) had been explored, it offered unlimited attractions for his special work. But as the journey out would be an expensive one, he was advised to lay his plans before Sir Roderick Murchison, then President of the Royal Geographical Society, and it was through his kindly interest and personal application to the Government that a passage was provided in one of the P. and O. boats going to Singapore. He left early in 1854. Arrived at Singapore, an entirely new world opened up before him. New peoples and customs thronged on all hands, a medley of nationalities such as can only be seen in the East, where, even to-day, and though forming part of one large community, each section preserves its native dress, customs and religious habits. After spending some time at Singapore he moved from place to place, but finally decided upon making Ternate his headquarters, as he discovered a comfortable bungalow, not too large, and adaptable in every way as a place in which to collect and prepare his specimens between the many excursions to other parts of the Archipelago. The name is now indelibly associated with that particular visit which ended after a trying journey in an attack of intermittent fever and general prostration, during which he first conceived the idea which has made Ternate famous in the history of natural science.

One or two points in the following letters recall certain contrasts similar to those already drawn between Darwin's impression of places and people and those made on the mind of Wallace by practically the same conditions. A typical instance is found in their estimate of the life and work of the missionaries whom they met and from whom they received the

warmest hospitality. Their experience included both Protestant and Roman Catholic, and from Darwin's account the former appeared to him to have the more civilising effect on the people, not only from a religious but also from the economic and industrial points of view.

In the "Journal" (p. 419) we find a detailed account of a visit to the missionary settlement at Waimate, New Zealand. After describing the familiar English appearance of the whole surroundings, he adds: "All this is very surprising when it is considered that five years ago nothing but the fern flourished here. Moreover, native workmanship, taught by these missionaries, has effected this change—the lesson of the missionary is the enchanter's wand. The house had been built, the windows framed, the field ploughed, and even the trees grafted, by the New Zealander. When I looked at the whole scene it was admirable. It was not that England was brought vividly before my mind; . . . nor was it the triumphant feeling at seeing what Englishmen could effect; but rather the high hopes thus inspired for the future progress of this fine island."

No such feeling was inspired by the conditions surrounding the Roman Catholic missionaries whom he met from time to time. In an earlier part of the "Journal" he records an evening spent with one living in a lonely place in South America who, "coming from Santiago, had contrived to surround himself with some few comforts. Being a man of some little education, he bitterly complained of the total want of society. With no particular zeal for religion, no business or pursuit, how completely must this man's life be wasted."

In complete opposition to these views, passages occur in the following letters which show that Wallace thought more highly of the Roman Catholic than of the Protestant missionaries. In one place, speaking of the former, he says: "Most are Frenchmen . . . well educated men who give up their lives for the good of the people they live among. I think Catholics and Protestants are equally wrong, but as missionaries I think Catholics are the best, and I would gladly see none others rather than have, as in New Zealand, sects of native Dissenters more rancorous against each other than in England. The unity of the Catholics is their strength, and an unmarried clergy can do as missionaries what married men never can undertake."

As a sidelight on these contradictory estimates of the same work, it should be borne in mind that Darwin had but recently given up the idea of becoming a clergyman, and doubtless retained some of the instinctive regard for sincere Christian Protestantism (whether represented by the Church of England or by Nonconformists), while Wallace had long since relinquished all doctrinal ideas on religion and all belief in the beneficial effect produced by such forms of worship on the individual.

Among the regions Wallace visited was Sarawak. Of one of his sojourns here some interesting reminiscences have been sent to me by Mr. L. V. Helmes. He says:

"It was in 1854 that Wallace came to Sarawak. I was there then, sent by a private firm, which later became the Borneo Company, to open up, by mining, manufacture and trade, the resources of the country, and amongst these enterprises was coal-mining on the west. Wallace came in search of new specimens of animal and especially insect life. The clearing of ancient forests at these mines offered a naturalist great opportunities, and I gave Wallace an introduction to our engineer in charge there. His collections of beetles and butterflies there were phenomenal; but the district was also the special home of the great ape, the orang-utan, or meias, as the natives called them, of which he obtained so many valuable specimens. Many notes must at that time have passed between us, for I took much interest in his work. We had put up a temporary hut for him at the mines, and on my occasional visits there I saw him and his young assistant, Charles Allan, at work, admired his beautiful collections, and gave my help in forwarding them.

"But it was mainly in social intercourse that we met, when Wallace, in intervals of his labours, came to Kuching, and was the Rajah's guest. Then occurred those interesting discussions at social gatherings to which he refers in a letter to me in 1909, when he wrote: 'I was pleased to receive your letter, with reminiscences of old times. I often recall those pleasant evenings with Rajah Brooke and our little circle, but since the old Rajah's death I have not met any of the party.'

"Wallace was in Sarawak at the happy period in the country's history. It was beginning to emerge from barbarism. The

Borneo Company was just formed, and the seed of the country's future prosperity was sown. Wallace, therefore, found us all sanguine and cheerful; yet we were on the brink of a disaster which brought many sorrows in its train. But the misfortunes of the Chinese revolt had not yet cast their shadows before them. The Rajah's white guests round his hospitable table; the Malay chiefs and office-holders, who made evening calls from curiosity or to pay their respects; Dyaks squatting in dusky groups in corners of the hall, with petitions to make or advice to seek from their white ruler—such would be the gathering of which Wallace would form a part. No suspicion or foreboding would trouble the company; yet within a few months that hall would be given to the flames of an enemy's torch, and the Rajah himself and many of those who formed that company would be fugitives in the jungle. . . .

“The Malay Archipelago, in the unregenerated days when Wallace roamed the forests and sailed the Straits in native boats and canoes, was full of danger to wanderers of the white race. Anarchy prevailed in many parts; usurping nobles enslaved the people in their houses; and piratical fleets scoured the sea, capturing and enslaving yearly thousands of peaceful traders, women and children. The writer was himself in 1862 besieged in a Bornean river by a pirate fleet, which was eventually destroyed by a Sarawak Government steamer with the following result of the fight: 190 pirates and 140 captives were killed or drowned, and 250 of the latter were liberated and sent to their homes; showing how formidable these pirates were. But Wallace, absorbed in his scientific pursuits, minded not these dangers, nor the hardships of any kind which a roving life in untrodden jungles and feverish swamps brings.

“When Wallace left Sarawak after his fifteen months' residence in the country, he left his young assistant, Charles Allan, there. He entered my service, and remained some time after the formation of the Borneo Company. Later, he again joined Wallace, and then went to New Guinea, doing valuable collecting and exploring work. He finally settled in Singapore, where I met him in 1899. He had married and was doing well; but died not long after my interview with him. He had come to the East with Wallace as a lad of 16, and had been his faithful companion and assistant during years of arduous work,”

The eight years spent by Wallace in this almost unknown part of the world were times of strenuous mental and physical exertion, resulting in the gathering together of an enormous amount of matter for future scientific investigation, but counter-balanced unfortunately by more or less continuous ill-health—which at times made the effort of clear reasoning and close application to scientific pursuits extremely difficult.

An indication of the unwearying application with which he went about his task is seen in the fact that during this period he collected 125,660 specimens of natural history, travelled about 14,000 miles within the Archipelago, and made sixty or seventy journeys, "each involving some preparation and loss of time," so that "not more than six years were really occupied in collecting."

A faint idea of this long and solitary sojourn in lonely places is given in a letter to his old friend Bates, dated December 24th, 1860, in which he says: "Many thanks for your long and interesting letter. I have myself suffered much in the same way as you describe, and I think more severely. The kind of *tædium vitæ* you mention I also occasionally experience here. I impute it to a too monotonous existence." And again when he begs his friend to write, as he is "half froze for news."

As already stated, Wallace, at no time during these wanderings, had any escort or protection, having to rely entirely upon his own tact and patience, combined with firmness, in his dealing with the natives. On one occasion he was taken ill, and had to remain six weeks with none but native Papuans around him, and he became so attached to them that when saying good-bye it was with the full intention of returning amongst them at a later period. In another place he speaks of sleeping under cover of an open palm-leaf hut as calmly as under the protection of the Metropolitan Police!

Up to that time, also, he was the only Englishman who had actually seen the beautiful "birds of paradise in their native forests," this success being achieved after "five voyages to different parts of the district they inhabit, each occupying in its preparation and execution the larger part of a year." And then only five species out of a possible fourteen were procured. His enthusiasm as a naturalist and collector knew no bounds, butterflies especially calling into play all his feelings of joy and satis-

faction. Describing his first sight of the *Ornithoptera cræsus*, he says that the blood rushed to his head and he felt much more like fainting than he had done when in apprehension of immediate death; a similar sensation being experienced when he came across another large bird-winged butterfly, *Ornithoptera poseidon*. "It is one thing," he says, "to see such beauty in a cabinet, and quite another to feel it struggling between one's fingers, and to gaze upon its fresh and living beauty, a bright green gem shining out amid the silent gloom of a dark and tangled forest. The village of Dobbo held that evening at least one contented man."

These thrills of joy may be considered as some compensation for such experiences as those contained in his graphic account of a single journey in a "prau," or native boat. "My first crew," he wrote, "ran away; two men were lost for a month on a desert island; we were ten times aground on coral reefs; we lost four anchors; our sails were devoured by rats; the small boat was lost astern; we were thirty-eight days on the voyage home which should have taken twelve; we were many times short of food and water; we had no compass-lamp owing to there not being a drop of oil in Waigiou when we left; and to crown it all, during the whole of our voyage, occupying in all seventy-eight days (all in what was supposed to be the favorable season), we had not one single day of fair wind."

The scientific discoveries arising out of these eight years of laborious work and physical hardship were first—with the exception of the memorable Essay on Natural Selection—included in his books on the Malay Archipelago, the Geographical Distribution of Animals, Island Life, and Australasia, besides a number of papers contributed to various scientific journals.

A bare catalogue of the places visited and explored includes Sumatra, Java, Borneo, Celebes, the Moluccas, Timor, New Guinea, the Aru and Ké Islands. Comparing this list with that given by Darwin at the close of the "Journal," we find that though in some respects the ground covered by the two men was similar, it never actually overlapped. The countries and islands visited by the *Beagle* came in the following order: Cape de Verde Islands, St. Paul's Rocks, Fernando Noronha, South America (including the Galapagos Archipelago, the Falkland Isles, and Tierra del Fuego), Tahiti, New Zealand, Australia,

Tasmania, Keeling Island, Maldivé Coral Atolls, Mauritius, St. Helena, Ascension. Brazil was revisited for a short time, and the *Beagle* touched at the Cape de Verde Islands and the Azores on the homeward voyage.

The very nature of this voyage did not permit Darwin to give unlimited time to the study of any particular spot or locality; but his accurate observation of every detail, together with his carefully kept journal, afforded ample scope and foundation for future contemplation. To Wallace, the outstanding result may be summed up in the fact that he discovered that the Malay Archipelago is divided into a western group of islands, which in their zoological affinities are Asiatic, and an eastern, which are Australian. The Oriental Borneo and Bali are respectively divided from the Australian Celebes and Lombok by a narrow belt of sea known as "Wallace's line," on the opposite side of which the indigenous mammalia are as widely divergent as in any two parts of the world.

To both men Darwin's estimate of the influence of travel may aptly apply in the sense that from a geographical point of view "the map of the world ceases to be a blank . . . each part assumes its proper dimensions," continents are no longer considered islands, nor islands as mere specks.

Wallace's homeward journey was not so eventful as the previous one had been, except for the unsuccessful efforts to bring back several species of live birds, which, with the exception of his birds of paradise, died on the way. On reaching London in the spring of 1862, he again made his home with his married sister, Mrs. Sims (who was living in Westbourne Grove). In a large empty room at the top of the house he found himself surrounded with packing-cases which he had not seen for five or six years, and which, together with his recent collections, absorbed his time and interest for the first few weeks. Later, he settled down to his literary work, and with the exception of one or two visits to the Continent and America, spent the remainder of his life in England—a life full of activity, the results of which still permeate scientific research.

A list of the principal works issued by him,¹ covering as they do subjects which apparently differ widely from one another,

¹ See Appendix.

affords something of a guide to matters which arise from time to time in the correspondence with his friends and fellow-scientists contained in the following pages. With this we bring to a conclusion a, perhaps, somewhat too lengthy introduction to the first group of letters. If any apology is needed, we venture to make it on behalf of those readers who may not have been previously acquainted with the details of Wallace's early life, or with the comparison here and there suggested between himself and Darwin which should be kept in mind in reading the letters that make up the larger portion of this volume.

PART I.—(*Continued*)

II.—Early Letters

(1854-62)

OF the few letters which have been preserved relating to this period, a number have already been published in "My Life" and need not be reprinted here. But in some cases portions of these letters have been given because they bring out aspects of Wallace's character which are not revealed elsewhere. The various omissions which have been made in other letters refer either to unimportant personal matters or to technical scientific details. The first of the letters was written during Wallace's voyage to the Malay Archipelago.

TO G. SILK

Steamer "Bengal," Red Sea. March 26 [1854].

My dear George,— . . . Of all the eventful days of my life my first in Alexandria was the most striking. Imagine my feelings when, coming out of the hotel (whither I have been conveyed in an omnibus) for the purpose of taking a quiet stroll through the city, I found myself in the midst of a vast crowd of donkeys and their drivers, all thoroughly determined to appropriate my person to their own use and interest, without in the least consulting my inclinations. In vain with rapid strides and waving arms I endeavoured to clear a way and move forward; arms and legs were seized upon, and even the Christian coat-tails were not sacred from the profane Mahometans. One would hold together two donkeys by their tails while I was struggling between them, and another, forcing together their heads, would thus hope to compel me to mount upon one or both of them; and one fellow more impudent than the rest I laid flat upon the ground, and sending the donkey staggering after him, I escaped a moment midst hideous yells and most unearthly cries. I now beckoned

to a fellow more sensible-looking than the rest, and told him that I wished to walk, and would take him for a guide, and hoped now to be at rest; but vain thought! I was in the hands of the Philistines, and getting us up against a wall, they formed an impenetrable phalanx of men and brutes thoroughly determined that I should only get away from the spot on the legs of a donkey. Bethinking myself now that donkey-riding was a national institution, and seeing a fat Yankee (very like my Paris friend) mounted, being like myself hopeless of any other means of escape, I seized upon a bridle in hopes that I should then be left in peace. But this was the signal for a more furious onset, for, seeing that I would at length ride, each one was determined that he alone should profit by the transaction, and a dozen animals were forced suddenly upon me and a dozen hands tried to lift me upon their respective beasts. But now my patience was exhausted, so, keeping firm hold of the bridle I had first taken with one hand, I hit right and left with the other, and calling upon my guide to do the same, we succeeded in clearing a little space around us. Now then behold your friend mounted upon a jackass in the streets of Alexandria, a boy behind holding by his tail and whipping him up, Charles (who had been lost sight of in the crowd) upon another, and my guide upon a third, and off we go among a crowd of Jews and Greeks, Turks and Arabs, and veiled women and yelling donkey-boys to see the city. We saw the bazaars and the slave market, where I was again nearly pulled to pieces for "backsheesh" (money), the mosques with their elegant minarets, and then the Pasha's new palace, the interior of which is most gorgeous.

We have seen lots of Turkish soldiers walking in comfortable irregularity; and after feeling ourselves to be dreadful guys for two hours, returned to the hotel whence we were to start for the canal boats. You may think this account is exaggerated, but it is not; the pertinacity, vigour and screams of the Alexandrian donkey-drivers no description can do justice to. . . .—Yours sincerely,

ALFRED R. WALLACE.

TO HIS MOTHER

Singapore. April 30, 1854.

My dear Mother,—We arrived here safe on the 20th of this month, having had very fine weather all the voyage. On shore

I was obliged to go to a hotel, which was very expensive, so I tried to get out into the country as soon as I could, which, however, I did not manage in less than a week, when I at last got permission to stay with a French Roman Catholic missionary who lives about eight miles out of the town and close to the jungle. The greater part of the inhabitants of Singapore are Chinese, many of whom are very rich, and all the villages about are almost entirely of Chinese, who cultivate pepper and gambir. Some of the English merchants here have splendid country houses. I dined with one to whom I brought an introduction. His house was most elegant, and full of magnificent Chinese and Japanese furniture. We are now at the Mission of Bukit Tima. The missionary speaks English, Malay and Chinese, as well as French, and is a very pleasant man. He has built a very pretty church here, and has about 300 Chinese converts. Having only been here four days, I cannot tell much about my collections yet. Insects, however, are plentiful. . . .

Charles gets on pretty well in health, and catches a few insects; but he is very untidy, as you may imagine by his clothes being all torn to pieces by the time we arrived here. He will no doubt improve and will soon be useful.

Malay is the universal language, in which all business is carried on. It is easy, and I am beginning to pick up a little, but when we go to Malacca shall learn it most, as there they speak nothing else.

I am very unfortunate with my watch. I dropped in on board and broke the balance-spring, and have now sent it home to Mr. Matthews to repair, as I cannot trust anyone here to do it. . . .

Love to Fanny and Thomas.—I remain, your affectionate son,
ALFRED R. WALLACE.

TO HIS MOTHER

Bukit Tama, Singapore. May 28, 1854.

My dear Mother,—I send you a few lines through G. Silk as I thought you would like to hear from me. I am very comfortable here living with a Roman Catholic missionary. . . . I send by this mail a small box of insects for Mr. Stevens. I think a very valuable one, and I hope it will go safely. I expected a

letter from you by the last mail, but received only two *Athenæums* of March 18 and 25.

The forest here is very similar to that of South America. Palms are very numerous, but they are generally small and horribly spiny. There are none of the large and majestic species so abundant on the Amazon. I am so busy with insects now that I have no time for anything else. I send now about a thousand beetles to Mr. Stevens, and I have as many other insects still on hand which will form part of my next and principal consignment. Singapore is very rich in beetles, and before I leave I think I shall have a most beautiful collection.

I will tell you how my day is now occupied. Get up at half-past five. Bath and coffee. Sit down to arrange and put away my insects of the day before, and set them safe out to dry. Charles mending nets, filling pincushions, and getting ready for the day. Breakfast at eight. Out to the jungle at nine. We have to walk up a steep hill to get to it, and always arrive dripping with perspiration. Then we wander about till two or three, generally returning with about 50 or 60 beetles, some very rare and beautiful. Bathe, change clothes, and sit down to kill and pin insects. Charles ditto with flies, bugs and wasps; I do not trust him yet with beetles. Dinner at four. Then to work again till six. Coffee. Read. If very numerous, work at insects till eight or nine. Then to bed.

Adieu, with love to all.—Your affectionate son,

ALFRED R. WALLACE.

TO HIS MOTHER

In the Jungle near Malacca. July, 1854.

My dear Mother,—As this letter may be delayed getting to Singapore I write at once, having an opportunity of sending to Malacca to-morrow. We have been here a week, living in a Chinese house or shed, which reminds me remarkably of my old Rio Negro habitation. I have now for the first time brought my "rede" into use, and find it very comfortable.

We came from Singapore in a small schooner with about fifty Chinese, Hindoos and Portuguese passengers, and were two days on the voyage, with nothing but rice and curry to eat, not having

made any provision, it being our first experience of these country vessels. Malacca is an old Dutch city, but the Portuguese have left the strongest mark of their possession in the common language of the place being still theirs. I have now two Portuguese servants, a cook and a hunter, and find myself thus almost brought back again to Brazil by the similarity of language, the people, and the jungle life. In Malacca we stayed only two days, being anxious to get into the country as soon as possible. I stayed with a Roman Catholic missionary; there are several here, each devoted to a particular part of the population, Portuguese, Chinese and wild Malays of the jungle. The gentleman we were with is building a large church, of which he is architect himself, and superintends the laying of every brick and the cutting of every piece of timber. Money enough could not be raised here, so he took a voyage *round the world!* and in the United States, California, and India got subscriptions sufficient to complete it.

It is a curious and not very creditable thing that in the English colonies of Singapore and Malacca there is not a single Protestant missionary; while the conversion, education and physical and moral improvement of the inhabitants (non-European) is entirely left to these French missionaries, who without the slightest assistance from our Government devote their lives to the Christianising and civilising of the varied populations which we rule over.

Here the bird sare abundant and most beautiful, more so than on the Amazon, and I think I shall soon form a most beautiful collection. They are, however, almost all common, and so are of little value except that I hope they will be better specimens than usually come to England. My guns are both very good, but I find powder and shot in Singapore cheaper than in London, so I need not have troubled myself to take any. So far both I and Charles have enjoyed excellent health. He can now shoot pretty well, and is so fond of it that I can hardly get him to do anything else. He will soon be very useful, if I can cure him of his incorrigible carelessness. At present I cannot trust him to do the smallest thing without watching that he does it properly, so that I might generally as well do it myself. I shall remain here probably two months, and then return to Singapore to prepare for a voyage to Cambodia or somewhere

else, so do not be alarmed if you do not hear from me regularly. Love to all.—Your affectionate son,

ALFRED R. WALLACE.

TO HIS MOTHER

Singapore. September 30, 1854.

My dear Mother,—I last wrote to you from Malacca in July. I have now just returned to Singapore after two months' hard work. At Malacca I had a pretty strong touch of fever with the old Rio Negro symptoms, but the Government doctor made me take a great quantity of quinine every day for a week together and so killed it, and in less than a fortnight I was quite well and off to the jungle again. I see now how to treat the fever, and shall commence at once when the symptoms again appear. I never took half enough quinine in America to cure me. Malacca is a pretty place, and I worked very hard. Insects are not very abundant there, still by perseverance I got a good number and many rare ones. Of birds, too, I made a good collection. I went to the celebrated Mount Ophir and ascended to the top. The walk was terrible—thirty miles through jungle, a succession of mud holes. My boots did good service. We lived there a week at the foot of the mountain in a little hut built by our men, and I got some fine new butterflies there and hundreds of other new and rare insects. We had only rice and a little fish and tea, but came home quite well. The height of the mountain is about 4,000 feet. . . . Elephants and rhinoceroses, as well as tigers, are abundant there, but we had our usual bad luck in not seeing any of them.

On returning to Malacca I found the accumulations of two or three posts, a dozen letters and fifty newspapers. . . .

I am glad to be safe in Singapore with my collections, as from here they can be insured. I have now a fortnight's work to arrange, examine, and pack them, and then in four months hence there will be some work for Mr. Stevens.

Sir James Brooke¹ is here. I have called on him. He received me most cordially, and offered me every assistance at

¹ The English Rajah of Sarawak—born 1803; appointed Rajah by the Sultan of Borneo, 1841; K. C. B. 1847; died 1868. He was succeeded as Rajah by his nephew, Sir Charles Johnson Brooke, G.C.M.J., born 1828.

Sarawak. I shall go there next, as the missionary does not go to Cambodia for some months. Besides, I shall have some pleasant society at Sarawak, and shall get on in Malay, which is very easy, but I have had no practice—though still I can ask for most common things. My books and instruments arrived in beautiful condition. They looked as if they had been packed up but a day. Not so the unfortunate eatables. . . . —I remain, your affectionate son,

ALFRED R. WALLACE.

TO G. SILK

Singapore. October 15, 1854.

Dear G.,—To-morrow I sail for Sarawak. Sir J. Brooke has given me a letter to his nephew, Capt. Brooke, to make me at home till he arrives, which may be a month, perhaps. I look forward with much interest to see what he has done and how he governs. I look forward to spending a very pleasant time at Sarawak. . . .

Sir W. Hooker's remarks are encouraging, but I cannot afford to collect plants. I have to work for a living, and plants would not pay unless I collect nothing else, which I cannot do, being too much interested in zoology. I should like a botanical companion like Mr. Spruce very much. We are anxiously expecting accounts of the taking of Sebastopol.

I am much obliged to Latham for quoting me, and hope to see it soon. That ought to make my name a little known. I have not your talent at making acquaintances, and find Singapore very dull. I have not found a single companion. I long for you to walk about with and observe the queer things in the streets of Singapore. The Chinamen and their ways are inexhaustibly amusing. My revolver is too heavy for daily use. I wish I had had a small one.—Yours sincerely,

ALFRED R. WALLACE.

TO AN UNKNOWN CORRESPONDENT ¹

Si Munjon Coal Works, Borneo. May, 1855.

One of the principal reasons which induced me to come here was that it is the country of those most strange and interesting

¹ This letter may have been written for publication,

animals, the orang-utans, or "mias" of the Dyaks. In the Sarawak district, though scarce twenty miles distant, they are quite unknown, there being some boundary line in this short space which, obeying the inexplicable laws of distribution, they never pass. The Dyaks distinguish three different kinds, which are known in Europe by skulls or skeletons only, much confusion still existing in their synonymy, and the external characters of the adult animals being almost or quite unknown. I have already been fortunate enough to shoot two young animals of two of the species, which were easily distinguishable from each other, and I hope by staying here some time to get adult specimens of all the species, and also to obtain much valuable information as to their habits. The jungle here is exceedingly monotonous; palms are scarce and flowers almost wanting, except some species of dwarf gingerwort. It is high on the trees that flowers are alone to be found. . . . Oak trees are rather plentiful, as I have already found three species with red, brown, and black acorns. This is confirmatory of Dr. Hooker's statement that, contrary to the generally received opinion, oaks are equally characteristic of a tropical as of a temperate climate. I must make an exception to the scarcity of flowers, however, tall slender trees occurring not unfrequently, whose stems are flower-bearing. One is a magnificent object, 12 or 15 ft. of the stem being almost hidden by rich orange-coloured flowers, which in the gloomy forest have, as I have before remarked of tropical insects under similar circumstances, an almost magical effect of brilliancy. Not less beautiful is another tree similarly clothed with spikes of pink and white berries.

The only striking features of the animal world are the horn-bills, which are very abundant and take the place of the toucans of Brazil, though I believe they have no real affinity with them; and the immense flights of fruit-eating bats which frequently pass over us. They extend as far as the eye can reach, and continue passing for hours. By counting and estimation I calculated that at least 30,000 passed one evening while we could see them, and they continued on some time after dark. The species is probably the *Pteropus edulis*; its expanded wings are near 5 ft. across, and it flies with great ease and rapidity. Fruit seems so scarce in these jungles that it is a mystery where they find enough to supply such vast multitudes,

Our mode of life here is very simple—rather too much so, as we have a continual struggle to get enough to eat. The Sarawak market is to a great extent supplied with rice, fowls, and sweet potatoes from this river, yet I have been obliged to send to Sarawak to purchase these very articles. The reason is that the Dyaks are almost all in debt to the Malay traders, and will therefore not sell anything, fearful of not having sufficient to satisfy their creditors. They have now just got in their rice harvest, and though it is not a very abundant one there is no immediate pressure of hunger to induce them to earn anything by hunting or snaring birds, etc. This also prevents them from being very industrious in seeking for the “mias,” though I have offered a high price for full-grown animals. The old men here relate with pride how many heads they have taken in their youth, and though they all acknowledge the goodness of the present Rajah’s government, yet they think that if they could still take a few heads, they would have better harvests. The more I see of uncivilised people, the better I think of human nature on the whole, and the essential differences between so-called civilised and savage man seem to disappear. Here are we, two Europeans surrounded by a population of Chinese, Malays, and Dyaks. The Chinese are generally considered, and with some truth, to be thieves, liars, and careless of human life, and these Chinese are coolies of the very lowest and least educated class. The Malays are invariably characterised as treacherous and blood-thirsty, and the Dyaks have only recently ceased to think head-taking an absolute necessity. We are two days’ journey from Sarawak, where, though the Government is European, yet it only exists by the consent and support of the native population. Now I can safely say that in any part of Europe, if the same facilities for crime and disturbance existed, things would not go on so smoothly as they do here. We sleep with open doors and go about constantly unarmed; one or two petty robberies and a little private fighting have taken place among the Chinese, but the great proportion of them are quiet, honest, decent sort of men. They did not at first like the strictness and punctuality with which the English manager kept them to their work, and two or three ringleaders tried to get up a strike for short hours and higher wages, but Mr. C.’s energy and decision soon stopped this by sending off the ringleaders at once, and summoning all

the Dyaks and Malays in the neighbourhood to his assistance in case of any resistance being attempted. It was very gratifying to see how rapidly they came up at his summons, and this display of power did much good, for since then everything has gone on smoothly. Preparations are now making for building a "joss house," a sure sign that the Chinese have settled to the work, and giving every promise of success in an undertaking which must have a vast influence on the progress of commerce and civilisation of Borneo and the surrounding countries. India, Australia, and every country with which they have communication must also be incalculably benefited by an abundant supply of good coal within two days' steam of Singapore. Let us wish success, then, to the Si Munjon Coal Works!—A. R. W.

TO HIS SISTER, MRS. SIMS

Sadong River. [Borneo.] June 25, 1855.

My dear Fanny,— . . . I am now obliged to keep fowls and pigs, or we should get nothing to eat. I have three pigs now and a China boy to attend to them, who also assists in skinning "orang-utans," which he and Charles are doing at this moment. I have also planted some onions and pumpkins, which were above ground in three days and are growing vigorously. I have been practising salting pork, and find I can make excellent pickled pork here, which I thought was impossible, as everyone I have seen try has failed. It is because they leave it to servants, who will not take the necessary trouble. I do it myself. I shall therefore always keep pigs in the future. I find there will not be time for another box round the Cape, so must have a small parcel overland. I should much like my *lasts*, but nothing else, unless some canvas shoes are made.

If the young man my mother and Mr. Stevens mentioned comes, he can bring them. I shall write to Mr. Stevens about the terms on which I can take him. I am, however, rather shy about it, having hitherto had no one to suit me. As you seem to know him, I suppose he comes to see you sometimes. Let me know what you think of him. Do not tell me merely that he is "a very nice young man." Of course he is. So is Charles a very nice boy, but I could not be troubled with another like him for any consideration whatever. I have written to Mr.

Stevens to let me know his character, as regards *neatness* and *perseverance* in doing anything he is set about. From you I should like to know whether he is quiet or boisterous, forward or shy, talkative or silent, sensible or frivolous, delicate or strong. Ask him whether he can live on rice and salt fish for a week on an occasion—whether he can do without wine or beer, and sometimes without tea, coffee or sugar—whether he can sleep on a board—whether he likes the hottest weather in England—whether he is too delicate to skin a stinking animal—whether he can walk twenty miles a day—whether he can work, for there is sometimes as hard work in collecting as in anything. Can he draw (not copy)? Can he speak French? Does he write a good hand? Can he make anything? Can he saw a piece of board straight? (Charles cannot, and every bit of carpenter work I have to do myself.) Ask him to make you anything—a little card box, a wooden peg or bottle stopper, and see if he makes them neat, straight and square. Charles never does anything the one or the other. Charles has now been with me more than a year, and every day some such conversation as this ensues: “Charles, look at these butterflies that you set out yesterday.” “Yes, sir.” “Look at that one—is it set out evenly?” “No, sir.” “Put it right then, and all the others that want it.” In five minutes he brings me the box to look at. “Have you put them all right?” “Yes, sir.” “There’s one with the wings uneven, there’s another with the body on one side, then another with the pin crooked. Put them all right this time.” It most frequently happens that they have to go back a third time. Then all is right. If he puts up a bird, the head is on one side, there is a great lump of cotton on one side of the neck like a wen, the feet are twisted soles uppermost, or something else. In everything it is the same, what ought to be straight is always put crooked. This after twelve months’ constant practice and constant teaching! And not the slightest sign of improvement. I believe he never will improve. Day after day I have to look over everything he does and tell him of the same faults. Another with a similar incapacity would drive me mad. He never, too, by any chance, puts anything away after him. When done with, everything is thrown on the floor. Every other day an hour is lost looking for knife, scissors, pliers, hammer, pins, or something he has mislaid. Yet out of doors he does very well—he collects

insects well, and if I could get a neat, orderly person in the house I would keep him almost entirely at out-of-door work and at skinning, which he does also well, but cannot put into shape. . . .
—Your affectionate brother,

ALFRED R. WALLACE.

TO HIS MOTHER

Sarawak. Christmas Day, 1855.

My dear Mother,—You will see I am spending a second Christmas Day with the Rajah. . . . I have lived a month with the Dyaks and have been a journey about sixty miles into the interior. I have been very much pleased with the Dyaks. They are a very kind, simple and hospitable people, and I do not wonder at the great interest Sir J. Brooke takes in them. They are more communicative and lively than the American Indians, and it is therefore more agreeable to live with them. In moral character they are far superior to either Malays or Chinese, for though head-taking has been a custom among them it is only as a trophy of war. In their own villages crimes are very rare. Ever since Sir J. has been here, more than twelve years, in a large population there has been but one case of murder in a Dyak tribe, and that one was committed by a stranger who had been adopted into the tribe. One wet day I got a piece of string to show them how to play "scratch cradle," and was quite astonished to find that they knew it better than I did and could make all sorts of new figures I had never seen. They were also very clever with tricks with string on their fingers, which seemed to be a favourite amusement. Many of the distant tribes think the Rajah cannot be a man. They ask all sorts of curious questions about him, whether he is not as old as the mountains, whether he cannot bring the dead to life, and I have no doubt for many years after his death he will be looked upon as a deity and expected to come back again. I have now seen a good deal of Sir James, and the more I see of him the more I admire him. With the highest talents for government he combines the greatest goodness of heart and gentleness of manner. At the same time he has such confidence and determination, that he has put down with the greatest ease some conspiracies of one or two Malay chiefs against him. It is a unique case in the

history of the world, for a European gentleman to rule over two conflicting races of semi-savages with their own consent, without any means of coercion, and depending solely upon them for protection and support, and at the same time to introduce the benefits of civilisation and check all crime and semi-barbarous practices. Under his government, "running amuck," so frequent in all other Malay countries, has never taken place, and with a population of 30,000 Malays, all of whom carry their "cruse" and revenge an insult by a stab, murders do not occur more than once in five or six years.

The people are never taxed but with their own consent, and Sir J.'s private fortune has been spent in the government and improvement of the country; yet this is the man who has been accused of injuring other parties for his own private interests, and of wholesale murder and butchery to secure his government! . . . —Your ever affectionate son,

ALFRED R. WALLACE.

TO HIS SISTER, MRS. SIMS

Singapore. February 20, 1856.

My dear Fanny,— . . . I have now left Sarawak, where I began to feel quite at home, and may perhaps never return to it again; but I shall always look back with pleasure to my residence there and to my acquaintance with Sir James Brooke, who is a gentleman and a nobleman in the noblest sense of both words. . . .

Charles has left me. He has stayed with the Bishop of Sarawak, who wants teachers, and is going to try to educate him for one. I offered to take him on with me, paying him a fair price for all the insects, etc., he collected, but he preferred to stay. I hardly know whether to be glad or sorry he has left. It saves me a great deal of trouble and annoyance, and I feel it quite a relief to be without him. On the other hand, it is a considerable loss for me, as he had just begun to be valuable in collecting. I must now try and teach a China boy to collect and pin insects. My collections in Borneo have been very good, but some of them will, I fear, be injured by the long voyages of the ships. I have collected upwards of 25,000 insects, besides birds, shells, quadrupeds, and plants. The day I arrived here a vessel sailed

for Macassar, and I fear I shall not have another chance for two months unless I go a roundabout way, and perhaps not then, so I have hardly made up my mind what to do.—Your affectionate brother,

ALFRED R. WALLACE.

TO HIS BROTHER-IN-LAW, THOMAS SIMS

Singapore. [Probably about March, 1856.]

Dear Thomas,— . . . You and Fanny talk of my coming back for a trifling sore as if I was within an omnibus ride of Conduit St. I am now perfectly well, and only waiting to go eastward. The far east is to me what the far west is to the Americans. They both meet in California, where I hope to arrive some day. I quite enjoy being a few days at Singapore now. The scene is at once so familiar and strange. The half-naked Chinese coolies, the neat shopkeepers, the clean, fat, old, long-tailed merchants, all as busy and full of business as any Londoners. Then the handsome Klings, who always ask double what they take, and with whom it is most amusing to bargain. The crowd of boatmen at the ferry, a dozen begging and disputing for a farthing fare, the Americans, the Malays, and the Portuguese make up a scene doubly interesting to me now that I know something about them and can talk to them in the general language of the place. The streets of Singapore on a fine day are as crowded and busy as Tottenham Court Road, and from the variety of nations and occupations far more interesting. I am more convinced than ever that no one can appreciate a new country in a short visit. After two years in the country I only now begin to understand Singapore and to marvel at the life and bustle, the varied occupations, and strange population, on a spot which so short a time ago was an uninhabited jungle. . . . —Yours affectionately,

ALFRED R. WALLACE.

TO HIS SISTER, MRS. SIMS

Singapore. April 21, 1856.

My dear Fanny,—I believe I wrote to you last mail, and have now little to say except that I am still a prisoner in Singapore and unable to get away to my land of promise, Macassar, with

whose celebrated oil you are doubtless acquainted. I have been spending three weeks with my old friend the French missionary, going daily into the jungle, and fasting on Fridays on omelet and vegetables, a most wholesome custom which I think the Protestants were wrong to leave off. I have been reading Huc's travels in China in French, and talking with a French missionary just arrived from Tonquin. I have thus obtained a great deal of information about these countries and about the extent of the Catholic missions in them, which is astonishing. How is it that they do their work so much more thoroughly than the Protestant missionaries? In Cochin China, Tonquin, and China, where all Christian missionaries are obliged to live in secret and are subject to persecution, expulsion, and often death, yet every province, even those farthest in the interior of China, have their regular establishment of missionaries constantly kept up by fresh supplies who are taught the languages of the countries they are going to at Penang or Singapore. In China there are near a million Catholics, in Tonquin and Cochin China more than half a million! One secret of their success is the cheapness of their establishments. A missionary is allowed about £30 a year, on which he lives, in whatever country he may be. This has two good effects. A large number of missionaries can be employed with limited funds, and the people of the countries in which they reside, seeing they live in poverty and with none of the luxuries of life, are convinced they are sincere. Most are Frenchmen, and those I have seen or heard of are well-educated men, who give up their lives to the good of the people they live among. No wonder they make converts, among the lower orders principally. For it must be a great comfort to these poor people to have a man among them to whom they can go in any trouble or distress, whose sole object is to comfort and advise them, who visits them in sickness, who relieves them in want, and whom they see living in daily danger of persecution and death only for their benefit.

You will think they have converted me, but in point of doctrine I think Catholics and Protestants are equally wrong. As missionaries I think Catholics are best, and I would gladly see none others, rather than have, as in New Zealand, sects of native dissenters more rancorous against each other than in England. The unity of the Catholics is their strength, and an

unmarried clergy can do as missionaries what married men can never undertake. I have written on this subject because I have nothing else to write about. Love to Thomas and Edward.—Believe me, dear Fanny, your ever affectionate brother,

ALFRED R. WALLACE.

TO HIS SISTER, MRS. SIMS

Macassar. December 10, 1856.

My dear Fanny,—I have received yours of September, and my mother's of October, and as I am now going out of reach of letters for six months I must send you a few lines to let you know that I am well and in good spirits, though rather disappointed with the celebrated Macassar. . . . For the last fortnight, since I came in from the country, I have been living here rather luxuriously, getting good rich cow's milk to my tea and coffee, very good bread and excellent Dutch butter (3s. a lb.). The bread here is raised with toddy just as it is fermenting, and it imparts a peculiar sweet taste to the bread which is very nice. At last, too, there is some fruit here. The mangoes have just come in, and they are certainly magnificent. The flavour is something between a peach and a melon, with the slightest possible flavour of turpentine, and very juicy. They say they are unwholesome, and it is a good thing for me I am going away now. When I come back there will be not one to be had. . . .—I remain, dear Fanny, your ever affectionate brother,

A. R. WALLACE.

H. W. BATES TO A. R. WALLACE

Tunantins, Upper Amazon, November 19, 1856.

Dear Wallace,— . . . I received about six months ago a copy of your paper in the *Annals* on "The Laws which have Governed the Introduction of New Species." I was startled at first to see you already ripe for the enunciation of the theory. You can imagine with what interest I read and studied it, and I must say that it is perfectly well done. The idea is like truth itself, so simple and obvious that those who read and understand it will be struck by its simplicity; and yet it is perfectly original. The reasoning is close and clear, and although so brief an essay,

it is quite complete, embraces the whole difficulty, and anticipates and annihilates all objections. Few men will be in a condition to comprehend and appreciate the paper, but it will infallibly create for you a high and sound reputation. The theory I quite assent to, and, you know, was conceived by me also, but I profess that I could not have propounded it with so much force and completeness.

Many details I could supply, in fact a great deal remains to be done to illustrate and confirm the theory: a new method of investigating and propounding zoology and botany inductively is necessitated, and new libraries will have to be written; in part of this task I hope to be a labourer for many happy and profitable years. What a noble subject would be that of a monograph of a group of beings peculiar to one region but offering different species in each province of it—tracing the laws which connect together the modifications of forms and colour with the *local* circumstances of a province or station—tracing as far as possible the actual *affiliation* of the species.

Two of such groups occur to me at once, in entomology, in Heliconiidae and Erotylidæ of South America; the latter I think more interesting than the former for one reason—the species are more local, having feebler means of locomotion than the Heliconiidae. . . . —Yours very truly,

HENRY WALTER BATES.

TO H. W. BATES

Amboyna. January 4, 1858.

My dear Bates,—My delay of six months in answering your very interesting and most acceptable letter dated Tunantins, 19th November, 1856, has not, I assure you, arisen either from laziness or indifference, but really from pressure of business and an unsettled state of mind. I received your letter at Macassar on my return in July last from a seven months' voyage and residence in the Aru Islands close to New Guinea. I found letters from Australia, California, yourself, Spruce, Darwin, home and a lot of interesting Stevensian dispatches. I had six months' collections (mostly in bad condition owing to dampness and sea air) to examine and pack; about 7,000 insects having to be gone over individually and many of them thoroughly cleaned, besides

an extensive collection of birds. I was thus occupied incessantly for a month, and then immediately left for a new locality in the interior, where I stayed three months, during which time I had most of my correspondence to answer, and was besides making some collections so curious and interesting that I did not feel inclined to answer your letter till I could tell you something about them.

At the end of October I returned to Macassar, packed up my collection, and left by steamer for Ternate, via this place, where I have stayed a month, had some good collecting, and it is now, on the day of my departure, having all my boxes packed and nothing to do, that I commence a letter to you.

Your letter has been a source of much pleasure and interest to me. I have read and re-read it at least twenty times. . . .

To persons who have not thought much on the subject I fear my paper on the succession of species will not appear so clear as it does to you. That paper is, of course, only the announcement of the theory, not its development. I have prepared the plan and written portions of an extensive work embracing the subject in all its bearings and endeavouring to prove what in the paper I have only indicated. It was the promulgation of Forbes's theory which led me to write and publish, for I was annoyed to see such an ideal absurdity put forth when such a simple hypothesis will explain *all the facts*.

I have been much gratified by a letter from Darwin, in which he says that he agrees with "almost every word" of my paper. He is now preparing for publication his great work on species and varieties, for which he has been collecting information twenty years. He may save me the trouble of writing the second part of my hypothesis by proving that there is no difference in nature between the origin of species and varieties, or he may give me trouble by arriving at another conclusion, but at all events his facts will be given for me to work upon. Your collections and my own will furnish most valuable material to illustrate and prove the universal applicability of the hypothesis. The connection between the succession of affinities and the geographical distribution of a group, worked out species by species, has never yet been shown as we shall be able to show it. In this Archipelago there are two distinct faunas rigidly circumscribed, which differ as much as those of South America and

Africa, and more than those of Europe and North America: yet there is nothing on the map or on the face of the islands to mark their limits. The boundary line often passes between islands closer than others in the same group. I believe the western part to be a separated portion of continental Asia, the eastern the fragmentary prolongation of a former Pacific continent. In mammalia and birds the distinction is marked by genera, families, and even orders confined to one region; in *insects* by a number of *genera* and little groups of peculiar species, the *families* of insects having generally a universal distribution.

Ternate. January 25, 1858.

I have not done much here yet, having been much occupied in getting a house repaired and put in order. This island is a volcano with a sloping spur on which the town is situated. About ten miles to the east is the coast of the large Island of Gilolo, perhaps the most perfect entomological *terra incognita* now to be found. I am not aware that a single insect has ever been collected there, and cannot find it given as the locality of any insects in my catalogues or descriptions. In about a week I go for a month collecting there, and then return to prepare for a voyage to New Guinea. I think I shall stay in this place two or three years, as it is the centre of a most interesting and almost unknown region. Every house here was destroyed in 1840 by an earthquake during an eruption of the volcano. . . .

What great political events have passed since we left England together! And the most eventful for England, and perhaps the most glorious, is the present mutiny in India, which has proved British courage and pluck as much as did the famed battles of Balaclava and Inkerman. I believe that both India and England will gain in the end by the fearful ordeal. When do you mean returning for good? If you go to the Andes you will, I think, be disappointed, at least in the number of species, especially of Coleoptera. My experience here is that the low grounds are much the most productive, though the mountains generally produce a few striking and brilliant species. . . . — Yours sincerely,

ALFRED R. WALLACE.

TO F. BATES

Ternate. March 2, 1858.

My dear Mr. Bates,—When I received your very acceptable letter (a month ago) I had just written one to your brother, which I thought I could not do better than send to you to forward to him, as I shall thereby be able to confine myself solely to the group you are studying and to other matters touched upon in your letter. I had heard from Mr. Stevens some time ago that you had begun collecting exotic Geodephaga, but were confining yourself to one or two illustrations of each genus. I was sure, however, that you would soon find this unsatisfactory. Nature must be studied in detail, and it is the wonderful variety of the species of a group, their complicated relations and their endless modification of form, size and colours, which constitute the pre-eminent charm of the entomologist's study. It is with the greatest satisfaction, too, I hail your accession to the very limited number of collectors and students of exotic insects, and sincerely hope you may be sufficiently favoured by fortune to enable you to form an extensive collection and to devote the necessary time to its study and ultimately to the preparation of a complete and useful work. Though I cannot but be pleased that you are able to do so, I am certainly surprised to find that you indulge in the expensive luxury of from three to seven specimens of a species. I should have thought that in such a very extensive group you would have found one or, at most, a pair quite sufficient. I fancy very few collectors of exotic insects do more than this, except where they can obtain additional specimens by gift or by exchange. Your remarks on my collections are very interesting to me, especially as I have kept descriptions with many outline figures of my Malacca and Sarawak Geodephaga, so that with one or two exceptions I can recognise and perfectly remember every species you mention. . . .

Now with regard to your request for notes of habits, etc. I shall be most willing to comply with it to some extent, first informing you that I look forward to undertaking on my return to England a "Coleoptera Malayana," to contain descriptions of the known species of the whole Archipelago, with an essay on their geographical distribution, and an account of the habits of the genera and species from my own observations. Of course,

therefore, I do not wish any part of my notes to be published, as this will be a distinctive feature of the work, so little being known of the habits, stations and modes of collecting exotic Coleoptera. . . .

You appear to consider the state of entomological literature flourishing and satisfactory: to *me* it seems quite the contrary. The number of unfinished works and of others with false titles is disgraceful to science. . . .

I think . . . on the whole we may say that the Archipelago is *very rich*, and will bear a comparison even with the richest part of South America. In the country between Ega and Peru there is work for fifty collectors for fifty years. There are hundreds and thousands of Andean valleys every one of which would bear exploring. Here it is the same with islands. I could spend twenty years here were life long enough, but feel I cannot stand it, away from home and books and collections and comforts, more than four or five, and then I shall have work to do for the rest of my life. What would be the use of accumulating materials which one could not have time to work up? I trust your brother may give us a grand and complete work on the Coleoptera of the Amazon Valley, if not of all South America. . . . —Yours faithfully,
ALFRED R. WALLACE.

TO HIS MOTHER

October 6, 1858.

My dear Mother,— . . . I have just returned from a short trip, and am now about to start on a longer one, but to a place where there are some soldiers, a doctor and engineer who speak English, so if it is good for collecting I shall stay there some months. It is Batchian, an island on the southwest side of Gilolo, about three or four days' sail from Ternate. I am now quite recovered from my New Guinea voyage and am in good health.

I have received letters from Mr. Darwin and Dr. Hooker, two of the most eminent naturalists in England, which has highly gratified me. I sent Mr. Darwin an essay on a subject on which he is now writing a great work. He showed it to Dr. Hooker and Sir C. Lyell, who thought so highly of it that they immediately read it before the Linnean Society. This assures me the acquaintance and assistance of these eminent men on my return home.

Mr. Stevens also tells me of the great success of the Aru collection, of which £1,000 worth has actually been sold. This makes me hope I may soon realise enough to live upon and carry out my long cherished plans of a country life in Old England.

If I had sent the large and handsome shells from Aru, which are what you expected to see, they would not have paid expenses, whereas the cigar box of small ones has sold for £50. You must not think I shall always do so well as at Aru; perhaps never again, because no other collections will have the novelty, all the neighbouring countries producing birds and insects very similar, and many even the very same. Still, if I have health I fear not to do very well. I feel little inclined now to go to California; as soon as I have finished my exploration of this region I shall be glad to return home as quickly and cheaply as possible. It will certainly be by way of the Cape or by second class overland. May I meet you, dear old Mother, and all my other relatives and friends in good health. Perhaps John and his trio will have had the start of me. . . .

TO H. W. BATES

Ceram. November 25, 1859.

Dear Bates,—Allow me to congratulate you on your safe arrival home with all your treasures; a good fortune which I trust is this time¹ reserved for me. I hope you will write to me and tell me your projects. Stevens hinted at your undertaking a "Fauna of the Amazon Valley." It would be a noble work, but one requiring years of labour, as of course you would wish to incorporate all existing materials and would have to spend months in Berlin and Milan and Paris to study the collections of Spix, Natterer, Oscolati, Castituan and others, as well as most of the chief private collections of Europe. I hope you may undertake it and bring it to a glorious conclusion. I have long been contemplating such a work for this Archipelago, but am convinced that the plan must be very limited to be capable of completion. . . . —I remain, dear Bates, yours very sincerely,

ALFRED R. WALLACE.

¹ Referring to the loss of his other collections. See p. 24.

TO H. W. BATES

Ternate. December 24, 1860.

Dear Bates,—Many thanks for your long and interesting letter. I have myself suffered much in the same way as you describe, and I think more severely. The kind of *tædium vitæ* you mention I also occasionally experience here. I impute it to a too monotonous existence.

I know not how or to whom to express fully my admiration of Darwin's book. To him it would seem flattery, to others self-praise; but I do honestly believe that with however much patience I had worked up and experimented on the subject, I could never have *approached* the completeness of his book—its vast accumulation of evidence, its overwhelming argument, and its admirable tone and spirit. I really feel thankful that it has not been left to me to give the theory to the public. Mr. Darwin has created a new science and a new philosophy, and I believe that never has such a complete illustration of a new branch of human knowledge been due to the labours and researches of a single man. Never have such vast masses of widely scattered and hitherto utterly disconnected facts been combined into a system, and brought to bear upon the establishment of such a grand and new and simple philosophy! . . .—In haste, yours faithfully,

ALFRED R. WALLACE.

TO HIS BROTHER-IN-LAW, THOMAS SIMS

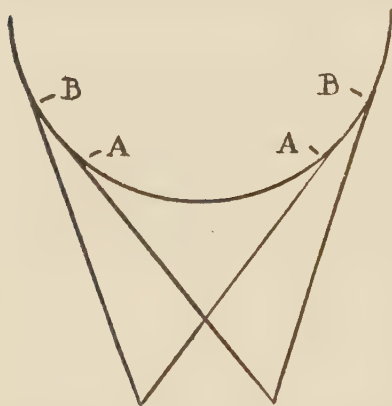
Delli, Timor. March 15, 1861.¹

My dear Thomas,—I will now try and write you a few lines in reply to your last three letters, which I have not before had time and inclination to do. First, about your *one-eyed* and *two-eyed* theory of art, etc., etc. I do not altogether agree with you. We do not see *all objects* wider with two eyes than with one. A spherical or curved object we do see so, because our right and left eye each sees a portion of the surface not seen by the other, but for that very reason the portion seen perfectly with both eyes is *less* than with one. Thus [see diagram on p. 60] we only see from A to A with both our eyes, the two side

¹ The original of this letter is in the possession of the Trustees of the British Museum.

portions AB AB being seen with but one eye, and therefore (when we are using both eyes) being seen obscurely. But if we look at a flat object, whether square or oblique to the line of vision, we see it of exactly the same size with two eyes as with one because the one eye can see no part of it that the other does not see also. But in painting I believe that this difference of proportion, where it does exist, is far too small to be *given* by any artist and also too small to affect the picture if given.

Again, I entirely deny that by *any means* the exact effect of a landscape with objects at various distances from the eye can be given on a flat surface; and moreover that the monocular clear outlined view is quite as true and good on the whole as the binocular hazy outlined view, and for this reason: we cannot and do not see clearly or look at two objects at once, if at different distances from us. In a real view our eyes are directed successively at every object, which we then see clearly and with distinct outlines, everything else—



nearer and farther—being indistinct; but being able to change the focal angle of our two eyes and their angle of direction with great rapidity, we are enabled to glance rapidly at each object in succession and thus obtain a general and detailed view of the whole. A house, a tree, a spire, the leaves of a shrub in the foreground, are each seen (while we direct our eyes to them) with perfect definition and sharpness of outline. Now a monocular photo gives the clearness of outline and accuracy of definition, and thus represents every individual part of a landscape just as we see it when looking at that part. Now I maintain that this is *right*; because no painting can represent an object both distinct and indistinct. The only question is, Shall a painting show us objects as we see them when looking at them, or as we see them when looking at *something else* near them? The only approach painters can make to this varying effect of binocular vision, and what they often

do, is to give the most important and main feature of their painting *distinct* as we should see it when looking at it in nature, while all around has a subdued tone and haziness of outline like that produced by seeing the real subjects when our vision is not absolutely directed to them. But then if, as in Nature, when you turn your gaze to one of these objects in order to see it clearly, you cannot do so, this is a defect. Again, I believe that we actually see in a good photograph better than in nature, because the best camera lenses are more perfectly adjusted than our eyes, and give objects at varying distances with better definition. Thus, in a picture we see at the same time near and distinct objects easily and clearly, which in reality we cannot do. If we could do so, everyone must acknowledge that our vision would be so much the more perfect and our appreciation of the beauties of nature more intense and complete; and in so far as a good landscape painting gives us this power it is better than nature itself; and I think this may account for that excessive and entrancing beauty of a good landscape or of a good panorama. You will think these ideas horribly heterodox, but if we all thought alike there would be nothing to write about and nothing to learn. I quite agree with you, however, as to artists using both eyes to paint and to see their paintings, but I think you quite mistake the theory of looking through the "catalogue"; it is not because the picture can be seen better with one eye, but because its effect can be better seen when all lateral objects are hidden—the catalogue does this. A double tube would be better, but that cannot be extemporised so easily. Have you ever tried a stereograph taken with the camera only the distance apart of the eyes? That must give *nature*. When the angle is greater the views in the stereoscope show us, not nature, but a perfect reduced model of nature seen nearer the eye.

It is curious that you should put Turner and the Pre-Raphaelites as *opposed* and representing *binocular* and *monocular* painting when Turner himself praises up the Pre-Raphaelites and calls Holman Hunt the greatest living painter!! . . .

Now for Mr. Darwin's book. You quite misunderstand Mr. D.'s statement in the preface and his sentiments. I have, of course, been in correspondence with him since I first sent him my little essay. His conduct has been most liberal and dis-

interested. I think anyone who reads the Linnean Society papers and his book will see it. I *do* back him up in his whole round of conclusions and look upon him as the *Newton of Natural History*.

You begin by criticising the *title*. Now, though I consider the title admirable, I believe it is not Mr. Darwin's, but the Publisher's, as you are no doubt aware that publishers *will* have a taking title, and authors must and do give way to them. Mr. D. gave me a different title before the book came out. Again, you misquote and misunderstand Huxley, who is a complete convert. Prof. Asa Gray and Dr. Hooker, the first two botanists of Europe and America, are converts. And Lyell, the first geologist living, who has all his life written against such conclusions as Darwin arrives at, is a convert and is about to declare or already has declared his conversion—a noble and almost unique example of a man yielding to conviction on a subject which he has taught as a master all his life, and confessing that he has all his life been wrong.

It is clear that you have not yet sufficiently read the book to enable you to criticise it. It is a book in which every page and almost every line has a bearing on the main argument, and it is very difficult to bear in mind such a variety of facts, arguments and indications as are brought forward. It was only on the *fifth* perusal that I fully appreciated the whole strength of the work, and as I had been long before familiar with the same subjects I cannot but think that persons less familiar with them cannot have any clear idea of the accumulated argument by a single perusal.

Your objections, so far as I can see anything definite in them, are so fully and clearly anticipated and answered in the book itself that it is perfectly useless my saying anything about them. He seems to me, however, as clear as daylight that the principle of natural selection *must* act in nature. It is almost as necessary a truth as any of mathematics. Next, the effects produced by this action *cannot be limited*. It cannot be shown that there *is* any limit to them in nature. Again, the millions of facts in the numerical relations of organic beings, their geographical distribution, their relations of affinity, the modification of their parts and organs, the phenomena of intercrossing, embryology and morphology—all are in accordance with his theory, and

almost all are necessary results from it; while on the other theory they are all isolated facts having no connection with each other and as utterly inexplicable and confusing as fossils are on the theory that they are special creations and are not the remains of animals that have once lived. It is the vast *chaos* of facts, which are explicable and fall into beautiful order on the one theory, which are inexplicable and remain a *chaos* on the other, which I think must ultimately force Darwin's views on any and every reflecting mind. Isolated difficulties and objections are nothing against this vast cumulative argument. The human mind cannot go on for ever accumulating facts which remain unconnected and without any mutual bearing and bound together by no law. The evidence for the production of the organic world by the simple laws of inheritance is exactly of the same nature as that for the production of the present surface of the earth—hills and valleys, plains, rocks, strata, volcanoes, and all their fossil remains—by the slow and natural action of natural causes now in operation. The mind that will ultimately reject Darwin must (to be consistent) reject Lyell also. The same arguments of apparent stability which are thought to disprove that organic species can change will also disprove any change in the inorganic world, and you must believe with your forefathers that each hill and each river, each inland lake and continent, were created as they stand, with their various strata and their various fossils—all appearances and arguments to the contrary notwithstanding. I can only recommend you to read again Darwin's account of the horse family and its comparison with pigeons; and if that does not convince and stagger you, then you are unconvertible. I do not expect Mr. Darwin's larger work will add anything to the general strength of his argument. It will consist chiefly of the details (often numerical) and experiments and calculations of which he has already given the summaries and results. It will therefore be more confusing and less interesting to the general reader. It will prove to scientific men the accuracy of his details, and point out the sources of his information, but as not one in a thousand readers will ever test these details and references the smaller work will remain for general purposes the best. . . .

I see that the Great Exhibition for 1862 seems determined on. If so it will be a great inducement to me to cut short the period

of my banishment and get home in time to see it. I assure you I now feel at times very great longings for the peace and quiet of home—very much weariness of this troublesome, wearisome, wandering life. I have lost some of that elasticity and freshness which made the overcoming of difficulties a pleasure, and the country and people are now too familiar to me to retain any of the charms of novelty which gild over so much that is really monotonous and disagreeable. My health, too, gives way, and I cannot now put up so well with fatigue and privations as at first. All these causes will induce me to come home as soon as possible, and I think I may promise, if no accident happens, to come back to dear and beautiful England in the summer of next year. C. Allen will stay a year longer and complete the work which I shall not be able to do.

I have been pretty comfortable here, having for two months had the society of Mr. Geach, a Cornish mining engineer who has been looking for copper here. He is a very intelligent and pleasant fellow, but has now left. Another Englishman, Capt. Hart, is a resident here. He has a little house on the foot of the hills two miles out of town; I have a cottage (which was Mr. Geach's) a quarter of a mile farther. He is what you may call a *speculative* man; he reads a good deal, knows a little and wants to know more, and is fond of speculating in the most abstruse and unattainable points of science and philosophy. You would be astonished at the number of men among the captains and traders of these parts who have more than an average amount to literary and scientific taste; whereas among the naval and military officers and various Government officials very few have any such taste, but find their only amusements in card-playing and dissipation. Some of the most intelligent and best informed Dutchmen I have met with are trading captains and merchants. This country much resembles Australia in its physical features, and is very barren compared with most of the other islands of the Archipelago. It is very rugged and mountainous, having no true forests, but a scanty vegetation of gum trees with a few thickets in moist places. It is consequently very poor in insects, and in fact will hardly pay my expenses; but having once come here I may as well give it a fair trial. Birds are tolerably abundant, but with few exceptions very dull coloured. I really believe the whole series of

birds of the tropical island of Timor are less beautiful and bright-coloured than those of Great Britain. In the mountains potatoes, cabbages, and wheat are grown in abundance, and so we get excellent pure bread made by Chinamen in Delli. Fowls, sheep, pigs and onions are also always to be had, so that it is the easiest country to live in I have yet met with, as in most other places one is always doubtful whether a dinner can be obtained. I have been a trip to the hills and stayed ten days in the clouds, but it was very wet, being the wrong season. . . .

Having now paid you off my literary debts, I trust you will give me credit again for some long letters on things in general. Address now to care of Hamilton, Gray and Co., Singapore, and with love and remembrances to all friends, I remain, my dear Thomas, yours very faithfully,

ALFRED R. WALLACE.

P.S.— . . . Will you, next time you visit my mother, make me a little plan of her cottage, showing the rooms and their dimensions, so that I may see if there will be room enough for me on my return? I shall want a good-sized room for my collections, and when I can decide exactly on my return it would be as well to get a little larger house beforehand if necessary. Please do not forget this.—Yours, A. R. W.

P.S.—Write by next mail, as circumstances have occurred which make it possible I may return home this year.—A. R. W.

P.S.—You allude in your last letter to a subject I never touch upon because I know we cannot agree upon it. However, I will now say a few words that you may know my opinions, and if you wish to convert me to your way of thinking, take more vigorous measures to effect it. You intimate that the happiness to be enjoyed in a future state will depend upon, and be a reward for, our belief in certain doctrines which you believe to constitute the essence of true religion. You must think, therefore, that belief is *voluntary* and also that it is *meritorious*. But I think that a little consideration will show you that belief is quite independent of our will, and our common expressions show it. We say, "I wish I could believe him innocent, but the evidence is too clear"; or, "Whatever people may say, I can never believe he can do such a mean action." Now, suppose in any similar

case the evidence on both sides leads you to a certain belief or disbelief, and then a reward is offered you for changing your opinion. Can you really change your opinion and belief, for the hope of reward or the fear of punishment? Will you not say, "As the matter stands I can't change my belief. You must give me proofs that I am wrong or show that the evidence I have heard is false, and then I may change my belief"? It may be that you do get more and do change your belief. But this change is not voluntary on your part. It depends upon the force of evidence upon your individual mind, and the evidence remaining the same and your mental faculties remaining unimpaired—you cannot believe otherwise any more than you can fly.

Belief, then is not voluntary. How, then, can it be meritorious? When a jury try a case, all hear the same evidence, but nine say "Guilty" and three "Not guilty," according to the honest belief of each. Are either of these more worthy of reward on that account than the others? Certainly you will say No! But suppose beforehand they all know or suspect that those who say "Not guilty" will be punished and the rest rewarded: what is likely to be the result? Why, perhaps six will say "Guilty" honestly believing it, and glad they can with a clear conscience escape punishment; three will say "Not guilty" boldly, and rather bear the punishment than be false or dishonest; the other three, fearful of being convinced against their will, will carefully stop their ears while the witnesses for the defence are being examined, and delude themselves with the idea they give an honest verdict because they have heard only one side of the evidence. If any out of the dozen deserve punishment, you will surely agree with me it is these. Belief or disbelief is therefore not meritorious, and when founded on an unfair balance of evidence is blameable.

Now to apply the principles to my own case. In my early youth I heard, as ninety-nine-hundredths of the world do, only the evidence on one side, and became impressed with a veneration for religion which has left some traces even to this day. I have since heard and read much on both sides, and pondered much upon the matter in all its bearings. I spent, as you know, a year and a half in a clergyman's family and heard almost every Tuesday the very best, most ear-

nest and most impressive preacher it has ever been my fortune to meet with, but it produced no effect whatever on my mind. I have since wandered among men of many races and many religions. I have studied man and nature in all its aspects, and I have sought after truth. In my solitude I have pondered much on the incomprehensible subjects of space, eternity, life and death. I think I have fairly heard and fairly weighed the evidence on both sides, and I remain an *utter disbeliever* in almost all that you consider the most sacred truths. I will pass over as utterly contemptible the oft-repeated accusation that sceptics shut out evidence because they will not be governed by the morality of Christianity. You I know will not believe that in my case, and *I* know its falsehood as a general rule. I only ask, Do you think I can change the self-formed convictions of twenty-five years, and could you think such a change would have anything in it to merit *reward* from *justice*? I am thankful I can see much to admire in all religions. To the mass of mankind religion of some kind is a necessity. But whether there be a God and whatever be His nature; whether we have an immortal soul or not, or whatever may be our state after death, I can have no fear of having to suffer for the study of nature and the search for truth, or believe that those will be better off in a future state who have lived in the belief of doctrines inculcated from childhood, and which are to them rather a matter of blind faith than intelligent conviction.—A. R. W.

This for yourself; show the *letter only* to my mother.

TO HIS MOTHER

Sourabaya, Java. July 20, 1861.

My dear Mother,—I am, as you will see, now commencing my retreat westwards, and have left the wild and savage Moluccas and New Guinea for Java, the garden of the East, and probably without any exception the finest island in the world. My plans are to visit the interior and collect till November, and then work my way to Singapore so as to return home and arrive in the spring. Travelling here will be a much pleasanter business than in any other country I have visited, as there are good roads, regular posting stages, and regular inns or lodging-houses all over the interior, and I shall no more be obliged to carry about with

me that miscellaneous lot of household furniture—bed, blankets, pots, kettles, and frying-pan, plates, dishes, and wash-basin, coffee-pots and coffee, tea, sugar, and butter, salt, pickles, rice, bread and wine, pepper and curry powder, and half a hundred more odds and ends, the constant looking after which, packing and re-packing, calculating and contriving, have been the standing plague of my life for the last seven years. You will better understand this when I tell you that I have made in that time about eighty movements averaging one a month, at every one of which all of these articles have had to be rearranged and repacked by myself according to the length of the trip, besides a constant personal supervision to prevent waste or destruction of stores in places where it is impossible to supply them.

Fanny wrote me last month to know about how I should like to live on my return. Of course, my dear mother, I should not think of living anywhere but with you, after such a long absence, if you feel yourself equal to housekeeping for us both; and I have always understood that your cottage would be large enough. The accommodation I should require is, besides a small bedroom, one large room, or a small one if there is, besides a kind of lumber room where I could keep my cases and do rough and dirty work. I expect soon from Thomas a sketch-plan of your cottage, by which I can at once tell if it will do. If not, I must leave you and Fanny to arrange as you like about a new residence. I should prefer being a little way out of town in a quiet neighbourhood and with a garden, but near an omnibus route, and if necessary I could lodge at any time for a week in London. This, I think, will be better and much cheaper than living close to town, and rents anywhere in the West End are sure now to rise owing to the approaching Great Exhibition. I must of course study economy, as the little money I have made will not be all got in for a year or two after my return. . . .

You must remember to write to me by the middle of November mail, as that is probably the last letter I can receive from you.

I send the letter to Fanny, who will most likely call on you and talk over matters. I am a little confused arriving in a new place with a great deal to do and living in a noisy hotel, so different to my usual solitary life, so that I cannot well collect my ideas to write any more, but must remain, my dear mother, your ever affectionate son,

ALFRED R. WALLACE.

TO HIS SISTER, MRS. SIMS

In the Mountains of Java. October 10, 1861.

My dear Fanny,—I have just received your second letter in praise of your new house. As I have said my say about it in my last, I shall now send you a few lines on other subjects.

I have been staying here a fortnight 4,000 feet above the sea in a fine cool climate, but it is unfortunately dreadfully wet and cloudy. I have just returned from a three days' excursion to one of the great Java volcanoes 10,000 feet high. I slept two nights in a house 7,500 feet above the sea. It was bitterly cold at night, as the hut was merely of plaited bamboo, like a sieve, so that the wind came in on all sides. I had flannel jackets and blankets and still was cold, and my poor men, with nothing but their usual thin cotton clothes, passed miserable nights lying on a mat on the ground round the fire which could only warm one side at a time. The highest peak is an extinct volcano with the crater nearly filled up, forming merely a saucer on the top, in which is a good house built by the government for the old Dutch naturalists who surveyed and explored the mountain. There are a lot of strawberries planted there, which do very well, but there were not many ripe. The common weeds and plants of the top were very like English ones, such as buttercups, sow-thistle, plantain, wormwood, chickweed, charlock, St. John's wort, violets and many others, all closely allied to our common plants of those names, but of distinct species. There was also a honeysuckle, and a tall and very pretty kind of cowslip. None of these are found in the low tropical lands, and most of them only on the tops of these high mountains. Mr. Darwin supposed them to have come there during a glacial or very cold period, when they could have spread over the tropics and, as the heat increased, gradually rose up the mountains. They were, as you may imagine, most interesting to me, and I am very glad that I have ascended *one* lofty mountain in the tropics, though I had miserable wet weather and had no view, owing to constant clouds and mist.

I also visited a semi-active volcano close by continually sending out steam with a noise like a blast-furnace—quite enough to give me a conception of all other descriptions of volcanoes.

The lower parts of the mountains of Java, from 3,000 to 6,000

feet, have the most beautiful tropical vegetation I have ever seen. Abundance of splendid tree ferns, some 50 ft. high, and some hundreds of varieties of other ferns, beautiful-leaved plants as begonias, melastomas, and many others, and more flowers than are generally seen in the tropics. In fact, this region exhibits all the beauty the tropics can produce, but still I consider and will always maintain that our own meadows and woods and mountains are more beautiful. Our own weeds and wayside flowers are far prettier and more varied than those of the tropics. It is only the great leaves and the curious-looking plants, and the deep gloom of the forests and the mass of tangled vegetation that astonish and delight Europeans, and it is certainly grand and interesting and in a certain sense beautiful, but not the calm, sweet, warm beauty of our own fields, and there is none of the brightness of our own flowers; a field of buttercups, a hill of gorse or of heather, a bank of foxgloves and a hedge of wild roses and purple vetches, surpass in *beauty* anything I have ever seen in the tropics. This is a favourite subject with me, but I cannot go into it now.

Send the accompanying note to Mr. Stevens immediately. You will see what I say to him about my collections here. Java is the richest of all the islands in birds, but they are as well known as those of Europe, and it is almost impossible to get a new one. However, I am adding fine specimens to my collection, which will be altogether the finest known of the birds of the Archipelago, except perhaps that of the Leyden Museum, who have had naturalists collecting for them in all the chief islands for many years with unlimited means.

Give my kind love to mother, to whom I will write next time.
—Your affectionate brother,

ALFRED R. WALLACE.

TO G. SILK ¹

Singapore. January 20, 1862.

My dear George,— . . . On the question of marriage we probably differ much. I believe a good wife to be the greatest blessing a man can enjoy, and the only road to happiness, but the

¹ For the other part of this letter see "My Life," i. 379.

qualifications I should look for are probably not such as would satisfy you. My opinions have changed much on this point: I now look at intellectual companionship as quite a secondary matter, and should my good stars ever send me an affectionate, good-tempered and domestic wife, I shall care not one iota for accomplishments or even for education.

I cannot write more now. I do not yet know how long I shall be here, perhaps a month. Then ho! for England!—In haste, yours most affectionately,

ALFRED R. WALLACE.

PART II

I.—The Discovery of Natural Selection

"There are not many joys in human life equal to the joy of the sudden birth of a generalisation, illuminating the mind after a long period of patient research. What has seemed for years so chaotic, so contradictory, and so problematic takes at once its proper position within an harmonious whole. Out of the wild confusion of facts and from behind the fog of guesses—contradicted almost as soon as they are born—a stately picture makes its appearance, like an Alpine chain suddenly emerging in all its grandeur from the mists which concealed it the moment before, glittering under the rays of the sun in all its simplicity and variety, in all its mightiness and beauty. And when the generalisation is put to a test, by applying it to hundreds of separate facts which seemed to be hopelessly contradictory the moment before, each of them assumes its due position, increasing the impressiveness of the picture, accentuating some characteristic outline, or adding an unsuspected detail full of meaning. The generalisation gains in strength and extent; its foundations grow in width and solidity; while in the distance, through the far-off mist on the horizon, the eye detects the outlines of new and still wider generalisations. He who has once in his life experienced this joy of scientific creation will never forget it; he will be longing to renew it; and he cannot but feel with pain that this sort of happiness is the lot of so few of us, while so many could also live through it—on a small or on a grand scale—if scientific methods and leisure were not limited to a handful of men."—PRINCE KROPOTKIN, "Memoirs of a Revolutionist."

THE social and scientific atmosphere in which Wallace found himself on his return from his eight years' exile in the Malay Archipelago was considerably more genial than that which he had enjoyed during his previous stay in London following his exploration of the Amazon. His position as one of the leading scientists of the day was already recognised, dating from the memorable 1st of July, 1858, when the two Papers, his own and Darwin's, on the theory of Natural Selection had been read before the Linnean Society.

During the four years which had elapsed since that date the

storm of criticism had waxed and waned; subsiding for a time only to burst out afresh from some new quarter where the theory bid fair to jeopardise some ancient belief in which scientist or theologian had rested with comparative satisfaction until so rudely disturbed.

During this period Wallace had been quietly pursuing his researches in the Malay Archipelago, though not without a keen interest in all that was taking place at home in so far as this reached him by means of correspondence and newspaper reports—his only means of keeping in touch with the world beyond the boundaries of the semi-civilised countries in which he was then living.

In order to follow the story of how the conception of the theory of Natural Selection grew and eventually took definite form in Wallace's mind, independently of the same development in the mind of Darwin, we must go back to a much earlier period in his life, and as nearly as possible link up the scattered remarks which here and there act as sign-posts pointing towards the supreme solution which has made his name famous for all time.

In Part I., Section I., many passages occur which clearly reveal his awakening to the study of nature. A chance remark overheard in conversation on the quiet street of Hertford touched the hidden spring of interest in a subject which was to become the one great purpose of his life. Then his enthusiastic yielding to the simple and natural attraction which flowers and trees have always exerted upon the sympathetic observer led step by step to the study of groups and families, until on his second sojourn at Neath, and about a year before his journey to South America with H. W. Bates, we find him deliberately pondering over the problem which many years later he described by saying that he "had in fact been bitten by the passion for species and their description."

In a letter to Bates dated November 9th, 1847, he concludes by asking, "Have you read 'Vestiges of the Natural History of Creation,' or is it out of your line?" and in the next (dated December 28th), in reply to one from his friend, he continues, "I have a rather more favourable opinion of the 'Vestiges' than you appear to have. I do not consider it a hasty generalisation, but rather an ingenious hypothesis strongly supported by some

striking facts and analogies, but which remains to be proved by more facts and the additional light which more research may throw upon the problem. . . . It furnishes a subject for every observer of nature to attend to; every fact," he observes, "will make either for or against it, and it thus serves both as an incitement to the collection of facts, and an object to which they can be applied when collected. Many eminent writers support the theory of the progressive development of animals and plants. There is a very philosophical work bearing directly on the question—Lawrence's 'Lectures on Man.' . . . The great object of these 'Lectures' is to illustrate the different races of mankind, and the manner in which they probably originated, and he arrives at the conclusion (as also does Prichard in his work on the 'Physical History of Man') that the varieties of the human race have not been produced by any external causes, but are due to the development of certain distinctive peculiarities in some individuals which have thereafter become propagated through an entire race. Now, I should say that a permanent peculiarity not produced by external causes is a characteristic of 'species' and not of mere 'variety,' and thus, if the theory of the 'Vestiges' is accepted, the Negro, the Red Indian, and the European are distinct species of the genus Homo.

"An animal which differs from another by some decided and permanent character, however slight, which difference is undiminished by propagation and unchanged by climate and external circumstances, is universally held to be a distinct *species*; while one which is not regularly transmitted so as to form a distinct race, but is occasionally reproduced from the parent stock (like albinos), is generally, if the difference is not very considerable, classed as a *variety*. But I would class both these as distinct *species*, and I would only consider those to be *varieties* whose differences are produced by external causes, and which, therefore, are not propagated as distinct races."

Again, writing about the same period, he adds: "I begin to feel rather dissatisfied with a mere local collection; little is to be learnt by it. I should like to take some one family to study thoroughly, principally with a view to the theory of the origin of species. By that means I am strongly of opinion that some definite results might be arrived at." And he further alludes

to "my favourite subject—the variations, arrangements, distribution, etc., of species."¹

It is evident that in Bates Wallace found his first real friend and companion in matters scientific; for in another letter he says: "I quite envy you, who have friends near you attracted to the same pursuits. I know not a single person in this little town who studies any one branch of natural history, so that I am quite alone in this respect." In fact, except for a little friendly help now and then, as in the case of Mr. Hayward lending him a copy of Loudon's *Encyclopædia of Plants*, he had always pondered over his nature studies without any assistance up to the time of his meeting Bates at Leicester.

From the date of this letter (1847) on to the early part of 1855—nearly ten years later—no reference is found either in his life or correspondence to the one absorbing idea towards which all his reflective powers were being directed. Then, during a quiet time at Sarawak, the accumulation of thought and observation found expression in an essay entitled "The Law which has Regulated the Introduction of Species," which appeared in the *Annals and Magazine of Natural History* in the following September (1855).

From November, 1854, the year of his arrival in the East, until January or February, 1856, Sarawak was the centre from which Wallace made his explorations inland, including some adventurous excursions on the Sadong River. During the wet season—or spring—of 1855, while living in a small house at the foot of the Santubong mountains (with one Malay boy who acted as cook and general companion), he tells us how he occupied his time in looking over his books and pondering "over the problem which was rarely absent from (his) thoughts." In addition to the knowledge he had acquired from reading such books as those by Swainson and Humboldt, also Lucien Bonaparte's "Conспектus," and several catalogues of insects and reptiles in the British Museum "giving a mass of facts" as to the distribution

¹ "My early letters to Bates suffice to show that the great problem of the origin of species was already distinctly formulated in my mind; that I was not satisfied with the more or less vague solutions at that time offered; that I believed the conception of evolution through natural law so clearly formulated in the 'Vestiges' to be, so far as it went, a true one; and that I firmly believed that a full and careful study of the facts of nature would ultimately lead to a solution of the mystery."—"My Life," i. 254-7.

of animals over the whole world, and having by his own efforts accumulated a vast store of information and facts direct from nature while in South America and since coming out East, he arrived at the conclusion that this "mass of facts" had never been properly utilised as an indication of the way in which species had come into existence. Having no fellow-traveller to whom he could confide these conclusions, he was almost driven to put his thoughts and ideas on paper—weighing each argument with studious care and open-eyed consideration as to its bearing on the whole theory. As the "result seemed to be of some importance," it was sent, as already mentioned, to the *Annals and Magazine of Natural History* as one of the leading scientific journals in England.

In the light of future events it is not surprising that Huxley (many years later) in referring to this "powerful essay," adds: "On reading it afresh I have been astonished to recollect how small was the impression it made."

As this earliest contribution by Wallace to the doctrine of Evolution is of peculiar historical value, and has not been so fully recognised as it undoubtedly deserves, and is now almost inaccessible, it will be useful to indicate in his own words the clear line of argument put forth by him two years before his second essay with which many readers are more familiar. He begins:¹

"Every naturalist who has directed his attention to the subject of the geographical distribution of animals and plants, must have been interested in the singular facts which it presents. Many of these facts are quite different from what would have been anticipated, and have hitherto been considered as highly curious, but quite inexplicable. None of the explanations attempted from the time of Linnæus are now considered at all satisfactory; none of them have given a cause sufficient to account for the facts known at the time, or comprehensive enough to include all the new facts which have since been, and are daily being added. Of late years, however, a great light has been thrown upon the subject by geological investigations, which have shown that the present state of the earth, and the organisms now inhabiting it, are but the last stage of a long and uninter-

¹ "On the Law which has regulated the Introduction of Species,"—*Ann. and Mag. of Natural History*, 2nd Series, p. 184.

rupted series of changes which it has undergone, and consequently, that to endeavour to explain and account for its present condition without any reference to those changes (as has frequently been done) must lead to very imperfect and erroneous conclusions. . . . The following propositions in Organic Geography and Geology give the main facts on which the hypothesis [see p. 96] is founded.

“GEOGRAPHY

“(1) Large groups, such as classes and orders, are generally spread over the whole earth, while smaller ones, such as families and genera, are frequently confined to one portion, often to a very limited district.

“(2) In widely distributed families the genera are often limited in range; in widely distributed genera, well-marked groups of species are peculiar to each geographical district.

“(3) When a group is confined to one district, and is rich in species, it is almost invariably the case that the most closely allied species are found in the same locality or in closely adjoining localities, and that therefore the natural sequence of the species by affinity is also geographical.

“(4) In countries of a similar climate, but separated by a wide sea or lofty mountains, the families, genera and species of the one are often represented by closely allied families, genera and species peculiar to the other.

“GEOLOGY

“(5) The distribution of the organic world in time is very similar to its present distribution in space.

“(6) Most of the larger and some of the smaller groups extend through several geological periods.

“(7) In each period, however, there are peculiar groups, found nowhere else, and extending through one or several formations.

“(8) Species of one genus, or genera of one family, occurring in the same geological time are more closely allied than those separated in time.

“(9) As generally in geography no species or genus occurs in two very distant localities without being also found in intermediate places, so in geology the life of a species or genus has

not been interrupted. In other words, no group or species has come into existence twice.

“(10) The following law may be deduced from these facts: *Every species has come into existence coincident both in time and space with a pre-existing closely allied species.*

“This law agrees with, explains and illustrates all the facts connected with the following branches of the subject: 1st, the system of natural affinities; 2nd, the distribution of animals and plants in space; 3rd, the same in time, including all the phenomena of representative groups, and those which Prof. Forbes supposed to manifest polarity; 4th, the phenomena of rudimentary organs. We will briefly endeavour to show its bearing upon each of these.

“If [this] law be true, it follows that the natural series of affinities will also represent the order in which the several species came into existence, each one having had for its immediate antetype a clearly allied species existing at the time of its origin . . . if two or more species have been independently formed on the plan of a common antetype, then the series of affinities will be compound, and can only be represented by a forked or many-branched line. . . . Sometimes the series of affinities can be well represented for a space by a direct progression from species to species or from group to group, but it is generally found impossible so to continue. There constantly occur two or more modifications of an organ or modifications of two distinct organs, leading us on to two distinct series of species, which at length differ so much from each other as to form distinct genera or families. These are the parallel series or representative groups and they often occur in different countries, or are found fossil in different formations. . . . We thus see how difficult it is to determine in every case whether a given relation is an analogy or an affinity, for it is evident that as we go back along the parallel or divergent series, towards the common antetype, the analogy which existed between the two groups becomes an affinity. . . . Again, if we consider that we have only the fragments of this vast system, the stems and main branches being represented by extinct species of which we have no knowledge, while a vast mass of limbs and boughs and minute twigs and scattered leaves is what we have to place in order, and determine the true position each originally occupied with regard to the others, the

whole difficulty of the true Natural System of classification becomes apparent to us.

"We shall thus find ourselves obliged to reject all those systems of classification which arrange species or groups in circles, as well as those which fix a definite number for the division of each group. . . . We have . . . never been able to find a case in which the circle has been closed by a direct affinity. In most cases a palpable analogy has been substituted, in others the affinity is very obscure or altogether doubtful. . . .

"If we now consider the geographical distribution of animals and plants upon the earth, we shall find all the facts beautifully in accordance with, and readily explained by, the present hypothesis. A country having species, genera, and whole families peculiar to it, will be the necessary result of its having been isolated for a long period sufficient for many series of species to have been created on the type of pre-existing ones, which, as well as many of the earlier-formed species, have become extinct, and made the groups appear isolated. . . .

"Such phenomena as are exhibited by the Galapagos Islands, which contain little groups of plants and animals peculiar to themselves, but most nearly allied to those of South America, have not hitherto received any, even a conjectural explanation. The Galapagos are a volcanic group of high antiquity and have probably never been more closely connected with the continent than they are at present."

He then proceeds at some length to explain how the Galapagos must have been at first "peopled . . . by the action of winds and currents," and that the modified prototype remaining are the "new species" which have been "created in each on the plan of the pre-existing ones." This is followed by a graphic sketch of the general effect of volcanic and other action as they affect the distribution of species, and the exact form in which they are found, even fishes giving "evidence of a similar kind: each great river (having) its peculiar genera, and in more extensive genera its groups of closely allied species."

After stating a number of practical examples he continues:

"The question forces itself upon every thinking mind—why are these things so? They could not be as they are, had no law regu-

lated their creation and dispersion. The law here enunciated not merely explains, but necessitates the facts we see to exist, while the vast and long-continued geological changes of the earth readily account for the exceptions and apparent discrepancies that here and there occur. The writer's object in putting forward his views in the present imperfect manner is to submit them to the tests of other minds, and to be made aware of all the facts supposed to be inconsistent with them. As his hypothesis is one which claims acceptance solely as explaining and connecting facts which exist in nature, he expects facts alone to be brought forward to disprove it, not *a priori* arguments against its probability."

He then refers to some of the geological "principles" expounded by Sir Charles Lyell on the "extinction of species," and follows this up by saying:

"To discover how the extinct species have from time to time been replaced by new ones down to the very latest geological period, is the most difficult, and at the same time the most interesting problem in the natural history of the earth. The present inquiry, which seeks to eliminate from known facts a law which has determined, to a certain degree, what species could and did appear at a given epoch, may, it is hoped, be considered as one step in the right direction towards a complete solution of it. . . . Admitted facts seem to show . . . a general, but not a detailed progression. . . . It is, however, by no means difficult to show that a real progression in the scale of organisation is perfectly consistent with all the appearances, and even with apparent retrogression should such occur."

Using once more the analogy of a branching tree to illustrate the natural arrangement of species and their successive creation, he clearly shows how "apparent retrogression may be in reality a progress, though an interrupted one"; as "when some monarch of the forest loses a limb, it may be replaced by a feeble and sickly substitute." As an instance he mentions the *Mollusca*, which at an early period had reached a high state of development of forms and species, while in each succeeding age modified species and genera replaced the former ones which had

become extinct, and "as we approach the present era but few and small representatives of the group remain, while the Gastropods and Bivalves have acquired an immense preponderance." In the long series of changes the earth had undergone, the process of peopling it with organic beings had been continually going on, and whenever any of the higher groups had become nearly or quite extinct, the lower forms which better resisted the modified physical conditions served as the antitype on which to found new races. In this manner alone, it was believed, could the representative groups of successive periods, and the risings and fallings in the scale of organisations, be in every case explained.

Again, alluding to a recent article by Prof. Forbes, he points out certain inaccuracies and how they may be proved to be so; and continues:

"We have no reason for believing that the number of species on the earth at any former period was much less than at present; at all events the aquatic portion with which the geologists have most acquaintance, was probably often as great or greater. Now we know that there have been many complete changes of species, new sets of organisms have many times been introduced in place of old ones which have become extinct, so that the total amount which have existed on the earth from the earliest geological period must have borne about the same proportion to those now living as the whole human race who have lived and died upon the earth to the population at the present time. . . . Records of vast geological periods are entirely buried beneath the ocean . . . beyond our reach. Most of the gaps in the geological series may thus be filled up, and vast numbers of unknown and unimaginable animals which might help to elucidate the affinities of the numerous isolated groups which are a perpetual puzzle to the zoologist may be buried there, till future revolutions may raise them in turn above the water, to afford materials for the study of whatever race of intelligent beings may then have succeeded us. These considerations must lead us to the conclusion that our knowledge of the whole series of the former inhabitants of the earth is necessarily most imperfect and fragmentary—as much as our knowledge of the present organic world would be, were we forced to make our collections

and observations only in spots equally limited in area and in number with those actually laid open for the collection of fossils. . . . The hypothesis of Prof. Forbes is essentially one that assumes to a great extent the *completeness* of our knowledge of the *whole series* of organic beings which have existed on earth. . . . The hypothesis put forward in this paper depends in no degree upon the completeness of our knowledge of the former condition of the organic world, but takes what facts we have as fragments of a vast whole, and deduces from them something of the nature and proportion of that whole which we can never know in detail. . . .

“Another important series of facts, quite in accordance with, and even necessary deductions from, the law now developed, are those of *rudimentary organs*. That these really do exist, and in most cases have no special function in the animal economy, is admitted by the first authorities in comparative anatomy. The minute limbs hidden beneath the skin in many of the snake-like lizards, the anal hooks of the boa constrictor, the complete series of jointed finger-bones in the paddle of the Manatee and the whale, are a few of the most familiar instances. In botany a similar class of facts has been long recognised. Abortive stamens, rudimentary floral envelope and undeveloped carpels are of the most frequent occurrence. To every thoughtful naturalist the question must arise, What are these for? What have they to do with the great laws of creation? Do they not teach us something of the system of nature? If each species has been created independently, and without any necessary relation with pre-existing species, what do these rudiments, these apparent imperfections mean? There must be a cause for them; they must be the necessary result of some great natural law. Now, if . . . the great law which has regulated the peopling of the earth with animal and vegetable life is, that every change shall be gradual; that no new creature shall be formed widely different from anything before existing; that in this, as in everything else in Nature, there shall be gradation and harmony—then these rudimentary organs are necessary and are an essential part of the system of Nature. Ere the higher vertebrates were formed, for instance, many steps were required, and many organs had to undergo modifications from the rudimental condition in which only they had as yet existed. . . . Many more of these modifica-

tions should we behold, and more complete series of them, had we a view of all the forms which have ceased to live. The great gaps that exist . . . would be softened down by intermediate groups, and the whole organic world would be seen to be an unbroken and harmonious system."

The article, in which we can see a great generalisation struggling to be born, ends thus:

"It has now been shown, though most briefly and imperfectly, how the law that 'every species has come into existence coincident both in time and space with a pre-existing closely allied species,' connects together and renders intelligible a vast number of independent and hitherto unexplained facts. The natural system of arrangement of organic beings, their geographical distribution, their geological sequence, the phenomena of representative and substituted groups in all their modifications, and the most singular peculiarities of anatomical structure, are all explained and illustrated by it, in perfect accordance with the vast mass of facts which the researches of modern naturalists have brought together, and, it is believed, not materially opposed to any of them. It also claims a superiority over previous hypotheses, on the ground that it not merely explains, but necessitates what exists. Granted the law, and many of the most important facts in Nature could not have been otherwise, but are almost as necessary deductions from it as are the elliptic orbits of the planets from the law of gravitation."

Some time after the appearance of this article, Wallace was informed by his friend and agent, Mr. Stevens, that several naturalists had expressed regret that he was "theorising," when what "was wanted was to collect more facts." Apart from this the only recognition which reached him in his remote solitude was a remark in an approving letter from Darwin, to which further allusion will be made presently.

As Wallace wrote nothing further of importance until the second essay which more fully disclosed his view of the origin of species, we will now briefly trace the growth of the theory of Natural Selection up to 1858, as it came to Darwin.

It is well known that during Darwin's voyage in the *Beagle*

he was deeply impressed by discovering extinct armadillo-like fossil forms in South America, the home of armadilloes, and by observing the relationship of the plants and animals of each island in the Galapagos group to those of the other islands and of South America, the nearest continent. These facts suggested evolution, and without evolution appeared to be meaningless.

Evolution and its motive cause were the problems which "haunted" him for the next twenty years. The first step towards a possible solution was the "opening of a notebook for facts in relation to the origin of species" in 1837, two years before the publication of his *Journal*. From the very commencement of his literary and scientific work, a rule rigidly adhered to was that of interspersing his main line of thought and research by reading books touching on widely diverging subjects; and it was thus, no doubt, that during October, 1838, he read "for amusement" Malthus's "Essay on Population"; not, as he himself affirms, with any definite idea as to its intimate bearing on the subject so near his heart. But the immediate result was that the idea of Natural Selection at once arose in his mind, and, in his own words, he "had a theory by which to work."

In May and June, 1842, during a visit to Maer and Shrewsbury, he wrote his first "pencil sketch of species theory," but not until two years later (1844) did he venture to enlarge this to one of 230 folio pages, "a wonderfully complete presentation of the arguments familiar to us in the 'Origin.'"¹

Already in addition to the mass of facts collected—and in course of being collected—by means of correspondence, paper-cuttings, and conversations with successful breeders, agricultural and horticultural experts, Darwin was busy with some of the experiments which he described in a letter to Sir Joseph Hooker (in 1855) as affording the latter a "good right to sneer, for they are so *absurd* even in *my* opinion that I dare not tell you." While a sentence in another letter (dated 1849) throws a side-light on all this preparatory work: "In your letter you wonder what 'ornamental poultry' has to do with barnacles; but do not flatter yourself that I shall not yet live to finish the barnacles, and then make a fool of myself on the subject of

¹ "Life of Charles Darwin" (one-vol. edit.), p. 171.

species, under which head ornamental poultry are very interesting."

Somewhere about this time (1842-44), Darwin, after referring to the vivid impression left on his mind by his reading Malthus on "Population" four years earlier, continues: "but at that time I overlooked one problem of great importance . . . the tendency in organic beings descended from the same stock to diverge in character as they become modified . . . and I can remember the very spot in the road, whilst in my carriage, when to my joy the solution occurred to me. . . . The solution, as I believe, is that the modified offspring of all dominant and increasing forms tend to become adapted to many and highly diversified places in the economy of nature."¹

So convinced was he of the truth of his ideas as expressed in the 1844 MS., that immediately after its completion he wrote the memorable letter to Mrs. Darwin telling her what he would wish done regarding its publication in the event of his death.

It was probably about two years later (1846) that he first confided his completed work—up to that date—to Sir Joseph Hooker, and later to Sir Charles Lyell; refraining, however, except in general conversation with other scientists, from informing anyone of the progress he was making towards a positive solution of the problem. His attitude of mind and manner at this period is happily illustrated by Huxley, who, speaking of his early acquaintance with Darwin, says: "I remember in the course of my first interview with Darwin expressing my belief in the sharpness of the line of demarcation between natural groups and in the absence of transitional forms, with all the confidence of youth and imperfect knowledge. I was not aware, at that time, that he had then been many years brooding over the species question; and the humorous smile which accompanied his gentle answer, that such was not altogether his view, long haunted and puzzled me."

Little did Charles Darwin dream that, only three years after this first MS. was written (in 1844), a youthful naturalist—known only as a surveyor at Neath—was deliberately pondering over the same issue, and writing to his only scientific friend on the subject. As, however, the different method of thought by

¹ "Life of Charles Darwin," p. 40.

which each arrived at the same conclusion is so aptly related by Wallace himself, we will leave it for him to tell the story in its appointed place.¹

In 1856, the year following the appearance of Wallace's essay in the *Annals and Magazine of Natural History*, both Hooker and Lyell urged Darwin to publish the result of his long and patient research. But he was still reluctant to do so, not having as yet satisfied himself with regard to certain conclusions which, he felt, must be stoutly maintained in face of the enormous amount of criticism which would arise immediately his theory was launched on the scientific world. And thus the event was postponed until the memorable year of 1858.

Up to the year 1856 no correspondence had passed between Wallace and Darwin, so far, at least, as the former could remember, for he says, in a letter dated Frith Hill, Godalming, December 3, 1887 (written to Mr. A. Newton): "I had hardly heard of Darwin before going to the East, except as connected with the voyage of the *Beagle*. . . . I saw him *once* for a few minutes in the British Museum before I sailed. Through Stevens, my agent, I heard that he wanted curious *varieties* which he was studying. I *think* I wrote about some varieties of ducks I had sent, and he must have written once to me. . . . But at that time I had not the remotest notion that he had already arrived at a definite theory—still less that it was the same as occurred to me, suddenly, in Ternate in 1858." It is clear, therefore, that the Essay written at Sarawak formed the first real link with Darwin, although not fully recognised at the time. In May, 1857, Darwin wrote to Wallace: "I am much obliged for your letter . . . and even still more by your paper in the *Annals*, a year or more ago. I can plainly see that we have thought much alike and to a certain extent have come to similar conclusions. . . . I agree to almost every word of your paper; and I daresay that you will agree with me that it is very rare to find oneself agreeing pretty closely with any theoretical paper." He concludes: "You have my very sincere and cordial good wishes for success of all kinds, and may all your theories succeed, except that on Oceanic Islands, on which subject I will do battle to the death."

¹ See p. 91-96.

The three years (from 1855-58), were for Wallace crowded with hard work, and perilous voyages by sea and hardships by land. January, 1858, found him at Amboyna, where, in all probability, he found a pile of long-delayed correspondence awaiting him, and among this a letter from Bates referring to the article which had appeared in print September, 1855. In reply he says: "To persons who have not thought much on the subject I fear my paper on the 'Succession of Species' will not appear so clear as it does to you. That paper is, of course, merely the announcement of the theory, not its development. I have prepared the plan and written portions of a work embracing the whole subject, and have endeavoured to prove in detail what I have as yet only indicated. . . . I have been much gratified by a letter from Darwin, in which he says that he agrees with 'almost every word' of my paper. He is now preparing his great work on 'Species and Varieties,' for which he has been preparing materials for twenty years. He may save me the trouble of writing more on my hypothesis, by proving that there is no difference in nature between the origin of species and of varieties; or he may give me trouble by arriving at another conclusion; but, at all events, his facts will be given for me to work upon. Your collections and my own will furnish most valuable material to illustrate and prove the universal application of the hypothesis. The connection between the succession of affinities and the geographical distribution of a group, worked out species by species, has never yet been shown as we shall be able to show it."

"This letter proves," writes Wallace,¹ "that at this time I had not the least idea of the nature of Darwin's proposed work nor of the definite conclusions he had arrived at, nor had I myself any expectations of a complete solution of the great problem to which my paper was merely the prelude. Yet less than two months later that solution flashed upon me, and to a large extent marked out a different line of work from that which I had up to this time anticipated. . . . In other parts of this letter I refer to the work I hoped to do myself in describing, cataloguing, and working out the distribution of my insects. I had in fact been bitten by the passion for species and their

¹ "My Life," i. 359.

description, and if neither Darwin nor myself had hit upon 'Natural Selection,' I might have spent the best years of my life in this comparatively profitless work. But the new ideas swept all this way."

This letter was finished after his arrival at Ternate, and a few weeks later he was prostrated by a sharp attack of intermittent fever which obliged him to take a prolonged rest each day, owing to the exhausting hot and cold fits which rapidly succeeded one another.

The little bungalow at Ternate had now come to be regarded as "home," for it was here that he stored all his treasured collections, besides making it the goal of all his wanderings in the Archipelago. One can understand, therefore, that in spite of the fever, there was a sense of satisfaction in the feeling that he was surrounded with the trophies of his arduous labours as a naturalist, and this passion for species and their descriptions being an ever-present speculation in his mind, his very surroundings would unconsciously conduce towards the line of thought which brought to memory the argument of "positive checks" set forth by Malthus in his "Principles of Population" (read twelve years earlier) as applied to savage and civilised races. "It then," he says, "occurred to me that these causes or their equivalents are continually acting in the case of animals also; and as animals usually breed much more rapidly than does mankind, the destruction every year from these causes must be enormous in order to keep down the numbers of each species, since they evidently do not increase regularly from year to year, as otherwise the world would have been densely crowded with those that breed most quickly. . . . Then it suddenly flashed upon me that this self-acting process would necessarily *improve the race*, because in every generation the inferior would inevitably be killed off and the superior would remain—that is, the *fittest would survive*. Then at once I seemed to see the whole effect of this, that when changes of land and sea, or of climate, or of food supply, or of enemies occurred—and we know that such changes have always been taking place—and considering the amount of individual variation that my experience as a collector had shown me to exist, then it followed that all the changes necessary for the adaptation of the species to the changing conditions would be brought about; and as great changes in the environment are

always slow, there would be ample time for the change to be effected by the survival of the best fitted in every generation. In this way every part of an animal's organism could be modified as required, and in the very process of this modification the unmodified would die out, and thus the *definite* characters and the clear *isolation* of each new species would be explained. The more I thought over it the more I became convinced that I had at length found the long-sought-for law of nature that solved the problem of the origin of species. For the next hour I thought over the deficiencies in the theories of Lamarck and of the author of the 'Vestiges,' and I saw that my new theory supplemented these views and obviated every important difficulty. I waited anxiously for the termination of my fit (of fever) so that I might at once make notes for a paper on the subject. The same evening I did this pretty fully, and on the two succeeding evenings wrote it out carefully in order to send it to Darwin by the next post, which would leave in a day or two." ¹

The story of the arrival of this letter at Down, and of the swift passage of events between the date on which Darwin received it and the reading of the "joint communications" before the Linnean Society, has been often told. But few, perhaps, have enjoyed the privilege of reading the account of this memorable proceeding as related by Sir Joseph Hooker at the celebration of the event, held by the Linnean Society in 1908.

As, therefore, the correspondence (pp. 105-262) between Wallace and Darwin during a long series of years conveys many expressions of their mutual appreciation of each other's work in connection with the origin of species, it will avoid a possible repetition of these if we take a long leap forward and give the notable speeches made by Dr. Wallace, Sir Joseph Hooker, Sir Ray Lankester, and others at this historical ceremony, which have not been published except in the *Proceedings* of the Society, now out of print.²

The gathering was held on July 1st, 1908, in the Institute of Civil Engineers, Great George Street, London, to celebrate the fiftieth anniversary of the joint communication made by Charles

¹ "My Life," i. 361-3.

² It will be remembered that Charles Darwin died in April, 1882, twenty-six years previously.

Darwin and Alfred Russel Wallace to the Linnean Society, "On the Tendency of Species to form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection." The large gathering included the President, Dr. Dukinfield H. Scott, distinguished representatives of many scientific Societies and Universities, Danish and Swedish Ministers, and a representative from the German Embassy. Most of the members of Dr. Wallace's and Mr. Darwin's family were also present. The President opened with some explanatory observations, and then invited Wallace to come forward in order to receive the first Darwin-Wallace Medal. In presenting it he said:

"Dr. Alfred Russel Wallace,—We rejoice that we are so happy as to have with us to-day the survivor of the two great naturalists whose crowning work we are here to commemorate.

"Your brilliant work in natural history and geography, and as one of the founders of the theory of Evolution by Natural Selection, is universally honoured and has often received public recognition, as in the awards of the Darwin and Royal Medals of the Royal Society, and of our medal in 1892.

"To-day in asking you to accept the first Darwin-Wallace Medal, we are offering you of your own, for it is you, equally with your great colleague, who created the occasion we celebrate.

"There is nothing in the history of science more delightful or more noble than the story of the relations between yourself and Mr. Darwin, as told in the correspondence now so fully published—the story of a generous rivalry in which each discoverer strives to exalt the claims of the other. We know that Mr. Darwin wrote (April 6th, 1859): 'You cannot tell how much I admire your spirit in the manner in which you have taken all that was done about publishing our papers. I had actually written a letter to you stating that I would not publish anything before you had published.' Then came the letters of Hooker and Lyell, leading to the publication of the joint papers which they communicated.

"You, on your side, always gave the credit to him, and underestimated your own position as the co-discoverer. I need only refer to your calling your great exposition of the joint theory 'Darwinism,' as the typical example of your generous emphasizing of the claims of your illustrious fellow-worker.

"It was a remarkable and momentous coincidence that both you and he should have independently arrived at the idea of Natural Selection after reading Malthus's book, and a most happy inspiration that you should have selected Mr. Darwin as the naturalist to whom to communicate your discovery. That theory, in spite of changes in the scientific fashion of the moment, you have always unflinchingly maintained, and still uphold as unshaken by all attacks.

"Like Mr. Darwin, you, if I may say so, are above all a naturalist, a student and lover of living animals and plants, as shown in later years by your enthusiasm and success in gardening. It is to such men, those who have learnt the ways of Nature, as Nature really is in the open, to whom your doctrine of Natural Selection specially appeals, and therein lies its great and lasting strength.

"Finally, you must allow me to allude to the generous interest you have always shown, and continue to show, in the careers of younger men who are endeavouring to follow in your steps.

"I ask you, Dr. Wallace, to accept this medal, struck in your honour and in that of the great work inaugurated fifty years ago by Mr. Darwin and yourself."

Wallace began his reply by thanking the Council of the Society for the honour they had done him, and then proceeded:

"Since the death of Darwin, in 1882, I have found myself in the somewhat unusual position of receiving credit and praise from popular writers under a complete misapprehension of what my share in Darwin's work really amounted to. It has been stated (not unfrequently) in the daily and weekly press, that Darwin and myself discovered 'Natural Selection' simultaneously, while a more daring few have declared that I was *the first* to discover it, and I gave way to Darwin!

"In order to avoid further errors of this kind (which this Celebration may possibly encourage), I think it will be well to give the actual facts as simply and clearly as possible.

"The *one fact* that connects me with Darwin, and which, I am happy to say, has never been doubted, is that the idea of what is now termed 'natural selection' or 'survival of the fittest,' together with its far-reaching consequences, occurred to us

independently, and was first jointly announced before this Society fifty years ago.

"But, what is often forgotten by the Press and the public is, that the idea occurred to Darwin in 1838, nearly twenty years earlier than to myself (in February, 1858); and that during the whole of that twenty years he had been laboriously collecting evidence from the vast mass of literature of biology, of horticulture, and of agriculture; as well as himself carrying out ingenious experiments and original observations, the extent of which is indicated by the range of subjects discussed in his 'Origin of Species,' and especially in that wonderful storehouse of knowledge, his 'Animals and Plants under Domestication,' almost the whole materials for which work had been collected, and to a large extent systematised, during that twenty years.

"So far back as 1844, at a time when I had hardly thought of any serious study of nature, Darwin had written an outline of his views, which he communicated to his friends Sir Charles Lyell and Dr. (now Sir Joseph) Hooker. The former strongly urged him to publish an abstract of his theory as soon as possible, lest some other person might precede him; but he always refused till he had got together the whole of the materials for his intended great work. Then, at last, Lyell's prediction was fulfilled, and, without any apparent warning, my letter, with the enclosed Essay, came upon him, like a thunderbolt from a cloudless sky! This forced him to what he considered a premature publicity, and his two friends undertook to have our two papers read before this Society.

"How different from this long study and preparation—this philosophical caution—this determination not to make known his fruitful conception till he could back it up by overwhelming proofs—was my own conduct.

"The idea came to me as it had come to Darwin, in a sudden flash of insight; it was thought out in a few hours—was written down with such a sketch of its various applications and developments as occurred to me at the moment—then copied on thin letter paper and sent off to Darwin—all within one week. I was then (as often since) the 'young man in a hurry': *he*, the painstaking and patient student seeking ever the full demonstration of the truth that he had discovered, rather than to achieve immediate personal fame.

"Such being the actual facts of the case, I should have had no cause for complaint if the respective shares of Darwin and myself in regard to the elucidation of nature's method of organic development had been henceforth estimated as being, roughly, proportional to the time we had each bestowed upon it when it was thus first given to the world—that is to say, as twenty years is to one week. For, he had already made it his own. If the persuasion of his friends had prevailed with him, and he had published his theory after ten years'—fifteen years'—or even eighteen years' elaboration of it—I should have had no part in it whatever, and *he* would have been at once recognised as the sole and undisputed discoverer and patient investigator of this great law of 'Natural Selection' in all its far-reaching consequences.

"It was really a singular piece of good luck that gave to me any share whatever in the discovery. During the first half of the nineteenth century (and even earlier) many great biological thinkers and workers had been pondering over the problem and had even suggested ingenious but inadequate solutions. Some of these men were among the greatest intellects of our time, yet, till Darwin, all had failed; and it was only Darwin's extreme desire to perfect his work that allowed me to come in, as a very bad second, in the truly Olympian race in which all philosophical biologists, from Buffon and Erasmus Darwin to Richard Owen and Robert Chambers, were more or less actively engaged.

"And this brings me to the very interesting question: Why did so many of the greatest intellects fail, while Darwin and myself hit upon the solution of this problem—a solution which this Celebration proves to have been (and still to be) a satisfying one to a large number of those best able to form a judgment on its merits? As I have found what seems to me a good and precise answer to this question, and one which is of some psychological interest, I will, with your permission, briefly state what it is.

"On a careful consideration, we find a curious series of correspondences, both in mind and in environment, which led Darwin and myself, alone among our contemporaries, to reach identically the same theory.

"First (and most important, as I believe), in early life both Darwin and myself became ardent beetle-hunters. Now there

is certainly no group of organisms that so impresses the collector by the almost infinite number of its specific forms, the endless modifications of structure, shape, colour, and surface-markings that distinguish them from each other, and their innumerable adaptations to diverse environments. These interesting features are exhibited almost as strikingly in temperate as in tropical regions, our own comparatively limited island-fauna possessing more than 3,000 species of this one order of insects.

"Again, both Darwin and myself had, what he terms 'the mere passion for collecting,' not that of studying the minutiae of structure, either internal or external. I should describe it rather as an intense interest in the variety of living things—the variety that catches the eye of the observer even among those which are very much alike, but which are soon found to differ in several distinct characters.

"Now it is this superficial and almost child-like interest in the outward forms of living things which, though often despised as unscientific, happened to be *the only one* which would lead us towards a solution of the problem of species. For nature herself distinguishes her species by just such characters—often exclusively so, always in some degree—very small changes in outline, or in the proportions of appendages, as give a quite distinct and recognisable *facies* to each, often aided by slight peculiarities in motion or habit; while in a larger number of cases differences of surface-texture, of colour, or in the details of the same general scheme of colour-pattern or of shading, give an unmistakable individuality to closely allied species.

"It is the constant search for and detection of these often unexpected differences between very similar creatures, that gives such an intellectual charm and fascination to the mere collection of these insects; and when, as in the case of Darwin and myself, the collectors were of a speculative turn of mind, they were constantly led to think upon the 'why' and the 'how' of all this wonderful variety in nature—this overwhelming, and, at first sight, purposeless wealth of specific forms among the very humblest forms of life.

"Then, a little later (and with both of us almost accidentally) we became travellers, collectors, and observers, in some of the richest and most interesting portions of the earth; and we thus had forced upon our attention all the strange phenomena of

local and geographical distribution, with the numerous problems to which they give rise. Thenceforward our interest in the great mystery of *how* species came into existence was intensified, and—again to use Darwin's expression—'haunted' us.

"Finally, both Darwin and myself, at the critical period when our minds were freshly stored with a considerable body of personal observation and reflection bearing upon the problem to be solved, had our attention directed to the system of *positive checks* as expounded by Malthus in his 'Principles of Population.' The effect of that was analogous to that of fiction upon the specially prepared match, producing that flash of insight which led us immediately to the simple but universal law of the 'survival of the fittest,' as the long sought *effective* cause of the continuous modification and adaptations of living things.

"It is an unimportant detail that Darwin read this book two years *after* his return from his voyage, while I read it *before* I went abroad, and it was a sudden recollection of its teachings that caused the solution to flash upon me. I attach much importance, however, to the large amount of solitude we both enjoyed during our travels, which, at the most impressionable period of our lives, gave us ample time for reflection on the phenomena we were daily observing.

"This view, of the combination of certain mental faculties and external conditions that led Darwin and myself to an identical conception, also serves to explain why none of our precursors or contemporaries hit upon what is really so very simple a solution of the great problem. Such evolutionists as Robert Chambers, Herbert Spencer, and Huxley, though of great intellect, wide knowledge, and immense power of work, had none of them the special turn of mind that makes the collector and the species-man; while they all—as well as the equally great thinker on similar lines, Sir Charles Lyell—became in early life immersed in different lines of research which engaged their chief attention.

"Neither did the actual precursors of Darwin in the statement of the principle—Wells, Matthews and Prichard—possess any adequate knowledge of the class of facts above referred to or sufficient antecedent interest in the problem itself, which were both needed in order to perceive the application of the principle to the mode of development of the varied forms of life.

“And now, to recur to my own position, I may be allowed to make a final remark. I have long since come to see that no one deserves either praise or blame for the *ideas* that come to him, but only for the actions resulting therefrom. Ideas and beliefs are certainly not voluntary acts. They come to us—we hardly know *how* or *whence*, and once they have got possession of us we cannot reject or change them at will. It is for the common good that the promulgation of ideas should be free—uninfluenced either by praise or blame, reward or punishment.

“But the *actions* which result from our ideas may properly be so treated, because it is only by patient thought and work that new ideas, if good and true, become adapted and utilised; while, if untrue or if not adequately presented to the world, they are rejected or forgotten.

“I therefore accept the crowning honour you have conferred on me to-day, not for the happy chance through which I became an independent originator of the doctrine of ‘survival of the fittest,’ but, as a too liberal recognition by you of the moderate amount of time and work I have given to explain and elucidate the theory, to point out some novel applications of it and (I hope I may add) for my attempts to extend those applications, even in directions which somewhat diverged from those accepted by my honoured friend and teacher Charles Darwin.”

Sir Joseph Hooker was now called upon by the President to receive the Darwin-Wallace Medal. In acknowledging the honour that had been paid him, he said:

“No thesis or subject was vouchsafed to me by the Council, but, having gratefully accepted the honour, I was bound to find one for myself. It soon dawned upon me that the object sought by my selection might have been that, considering the intimate terms upon which Mr. Darwin extended to me his friendship, I could from my memory contribute to the knowledge of some important events in his career. It having been intimated to me that this was in a measure true, I have selected as such an event one germane to this Celebration and also engraven on my memory, namely, the considerations which determined Mr. Darwin to assent to the course which Sir Charles Lyell and

myself had suggested to him, that of presenting to the Society, in one communication, his own and Mr. Wallace's theories on the effect of variation and the struggle for existence on the evolution of species.

"You have all read Francis Darwin's fascinating work as Editor of his father's 'Life and Letters,' where you will find (Vol. II., p. 116) a letter addressed, on the 18th of June, 1858, to Sir Charles Lyell by Mr. Darwin, who states that he had on that day received a communication from Mr. Wallace written from the Celebes Islands requesting that it might be sent to him (Sir Charles).

"In a covering letter Mr. Darwin pointed out that the enclosure contained a sketch of a theory of Natural Selection as depending on the struggle for existence so identical with one he himself entertained and fully described in MS. in 1842 that he never saw a more striking coincidence: had Mr. Wallace seen his sketch he could not have made a better short abstract, even his terms standing 'as heads of chapters.' He goes on to say that he would at once write to Mr. Wallace offering to send his MS. to any journal; and concludes: 'So my originality is smashed, though my book [the forthcoming "Origin of Species"], if it will have any value will not be deteriorated, as all know the labour consists in the application of the theory.'

"After writing to Sir Charles Lyell, Mr. Darwin informed me of Mr. Wallace's letter and its enclosure, in a similar strain, only more explicitly announcing his resolve to abandon all claim to priority for his own sketch. I could not but protest against such a course, no doubt reminding him that I had read it, and that Sir Charles knew its contents some years before the arrival of Mr. Wallace's letter; and that our withholding our knowledge of its priority would be unjustifiable. I further suggested the simultaneous publication of the two, and offered—should he agree to such a compromise—to write to Mr. Wallace fully informing him of the motives of the course adopted.

"In answer Mr. Darwin thanked me warmly for my offer to explain all to Mr. Wallace, and in a later letter he informed me that he was disposed to look favourably on my suggested compromise, but that before making up his mind he desired a second opinion as to whether he could honourably claim priority, and that he proposed applying to Sir Charles Lyell for this. I need

not say that this was a relief to me, knowing as I did what Sir Charles's answer must be.

"In Vol. II., pp. 117-8, of the 'Life and Letters,' Mr. Darwin's application to Sir Charles Lyell is given, dated June 26th, with a postscript dated June 27th. In it he requests that the answer shall be sent to me to be forwarded to himself. I have no recollection of reading the answer, which is not to be found either in Darwin's or my own correspondence; it was no doubt satisfactory.

"Further action was now left in the hands of Sir Charles and myself, we all agreeing that whatever action was taken, the result should be offered for publication to the Linnean Society.

"On June 29th Mr. Darwin wrote to me in acute distress, being himself very ill, and scarlet fever raging in the family, to which one infant son had succumbed on the previous day, and a daughter was ill with diphtheria. He acknowledged the receipt of the letter from me, adding, 'I cannot think now of the subject, but soon will: you shall hear as soon as I can think'; and on the night of the same day he writes again, telling me that he is quite prostrated and can do nothing but send certain papers for which I had asked as essential for completing the prefatory statement to the communication to the Linnean Society of Mr. Wallace's Essays. . . .

"The communications were read, as was the custom in those days, by the Secretary to the Society. Mr. Darwin himself, owing to his illness and distress, could not be present. Sir Charles Lyell and myself said a few words to emphasise the importance of the subject, but, as recorded in the 'Life and Letters' (Vol. II., p. 126), although intense interest was excited, no discussion took place: 'the subject was too novel, too ominous, for the old school to enter the lists before armouring.'

"It must also be noticed that for the detailed history given above there is no documentary evidence beyond what Francis Darwin has produced in the 'Life and Letters.' There are no letters from Lyell relating to it, not even answers to Mr. Darwin's of the 18th, 25th, and 26th of June; and Sir Leonard Lyell has at my request very kindly but vainly searched his uncle's correspondence for any relating to this subject beyond the two above mentioned. There are none of my letters to either Lyell or Darwin, nor other evidence of their having existed beyond

the latter's acknowledgment of the receipt of some of them; and, most surprising of all, Mr. Wallace's letter and its enclosure have disappeared. Such is my recollection of this day, the fiftieth anniversary of which we are now celebrating, and of the fortnight that immediately preceded it.

"It remains for me to ask your forgiveness for intruding upon your time and attention with the half-century-old real or fancied memories of a nonagenarian as contributions to the history of the most notable event in the annals of Biology that had followed the appearance in 1735 of the '*Systema Naturæ*' of Linnæus."

Following Sir J. Hooker, the President, referring to Prof. Haeckel, who was unable to attend, said that "he was the great apostle of the Darwin-Wallace theory in Germany . . . his enthusiastic and gallant advocacy (having) chiefly contributed to its success in that country. . . . A man of world-wide reputation, the leader on the Continent of the 'Old Guard' of evolutionary Biologists, Prof. Haeckel was one whom the Linnean Society delighted to honour."

Two more German scientists were also honoured with the Medal, namely Prof. August Weismann, who was also absent, and Prof. Eduard Strasburger, the latter paying a special tribute to Wallace in saying: "When I was young the investigations and the thought of Alfred Russel Wallace brought me a great stimulus. Through his '*Malay Archipelago*' a new world of scientific knowledge was unfolded before me. On this occasion I feel it my duty to proclaim it with gratitude." The Medal was then presented to Sir Francis Galton, who delivered a notable speech in responding.

The last on this occasion to receive the Medal was Sir E. Ray Lankester, who, in replying to the President's graceful speech, referred to the happy relationships which had existed between the contemporary men of science of his own time, but with special reference to Charles Darwin and Alfred Russel Wallace he said:

"Never was there a more beautiful example of modesty, of unselfish admiration for another's work, of loyal determination that the other should receive the full merit of his independent labours and thoughts, than was shown by Charles Darwin on that occasion. . . .

"Subsequently, throughout all their arduous work and varied publications upon the great doctrine which they on that day unfolded to humanity . . . the same complete absence of rivalry characterised these high-minded Englishmen, even when in some outcomes of their doctrine they were not in perfect agreement. . . . I think I am able to say that great as was the interest excited by the new doctrine in the scientific world, and wild and angry as was the opposition to it in some quarters, few, if any, who took part in the scenes attending the birth and earlier reception of Darwin's 'Origin of Species' had a pre-vision of the enormous and all-important influence which that doctrine was destined to exercise upon every line of human thought. . . . It is in its application to the problems of human society that there still remains an enormous field of work and discovery for the Darwin-Wallace doctrine.

"In the special branch of study which Wallace himself set going—the inquiry into the local variations, races, and species of insects as evidence of descent with modification, and of the mechanism by which that modification is brought about—there is still great work in progress, still an abundant field to be reaped. . . . Several able observers and experimenters have set themselves the task of improving, if possible, the theoretical structure raised by Darwin and Wallace. . . . But I venture to express the opinion that they have none of them resulted in any serious modification of the great doctrine submitted to the Linnean Society on July 1st, 1858, by Charles Darwin and Alfred Russel Wallace. Not only do the main lines of the theory of Darwin and Wallace remain unchanged, but the more it is challenged by new suggestions and new hypotheses the more brilliantly do the novelty, the importance, and the permanent value of the work by those great men, to-day commemorated by us, shine forth as the one great epoch-making effort of human thought on this subject."

Sir Francis Darwin and Sir William Thiselton-Dyer spoke on behalf of schools who had sent representatives to the meeting: Prof. Lönnberg and Sir Archibald Geikie on behalf of the Academies and Societies: while Lord Avebury delivered the concluding address.

Any summary of this period in the lives of Darwin and Wallace would be incomplete without some distinct reference to one other

name, namely, that of Herbert Spencer, whom I have linked with them in the Introduction.

While we owe to Darwin and Wallace a definite theory of organic development, it must be remembered that Spencer included this in the general scheme of evolution which grew as slowly but surely in his mind—and as independently as did that of the origin of species in the minds of Darwin and Wallace. Huxley recalls: "Within the ranks of biologists, at that time, I met with nobody except Dr. Grant, of University College, who had a word to say for Evolution—and his advocacy was not calculated to advance the cause. Outside these ranks, the only person known to me whose knowledge and capacity compelled respect, and who was, at the same time, a thoroughgoing evolutionist, was Mr. Herbert Spencer. . . . Many and prolonged were the battles we fought on this topic. . . . I took my stand upon two grounds: first, that up to that time, the evidence in favour of transmutation was wholly insufficient; and, secondly, that no suggestions respecting the causes of the transmutations assumed . . . were in any way adequate to explain the phenomena. Looking back at the state of knowledge at that time, I really do not see that any other conclusion was justifiable." ¹

And Prof. Raphael Meldola, in a lecture on Evolution where-in he compares the impression left by each of these great founders of that school upon the current of modern thought, says: "Through all . . . his [Spencer's] writings the underlying idea of development can be traced with increasing depth and breadth, expanding in 1850 in his 'Social Statics' to a foreshadowing of the general doctrine of Evolution. In 1852 his views on organic evolution had become so definite that he gave public expression to them in that well-known and powerful essay on 'The Development of Hypothesis.' . . . In the 'Principles of Psychology,' the first edition of which was published in 1855, the evolutionary principle was dominant. By 1858—the year of the announcement of Natural Selection by Darwin and Wallace—he had conceived the great general scheme and had sketched out the first draft of the prospectus of the Synthetic Philosophy, the final and amended syllabus [being] issued in 1860. The work of Darwin and Spencer from that period, although moving along in-

¹ "Life and Letters of Charles Darwin," ii. 188.

dependent lines, was directed towards the same end, notwithstanding the diversity of materials which they made use of and the differences in their methods of attack; that end was the establishment of Evolution as a great natural principle or law."¹

In this connection it is especially interesting to note how near Spencer had come to the conception of Natural Selection without grasping its full significance. In an article on a "Theory of Population" (published in the *Westminster Review* for April, 1852) he wrote: "And here, indeed, without further illustration, it will be seen that premature death, under all its forms and from all its causes, cannot fail to work in the same direction. For as those prematurely carried off must, in the average of cases, be those in whom the power of self-preservation is the least, it unavoidably follows that those left behind to continue the race must be those in whom the power of self-preservation is the greatest—must be the select of their generation. So that whether the dangers of existence be of the kind produced by excess of fertility, or of any other kind, it is clear that by the ceaseless exercise of the faculties needed to contend with them, and by the death of all men who fail to contend with them successfully, there is ensured a constant progress towards a higher degree of skill, intelligence, self-regulation—a better co-ordination of actions—a more complete life."

Up to the period of the publication of the "Origin of Species" and the first conception of the scheme of the Synthetic Philosophy there had been no communication between Darwin and Spencer beyond the presentation by Spencer of a copy of his *Essays* to Darwin in 1858, which was duly acknowledged. But by the time the "Origin of Species" had been before the public for eight years, the Darwinian principle of selection had become an integral part of the Spencerian mechanism of organic evolution. Indeed the term "survival of the fittest," approved by both Darwin and Wallace as an alternative for "natural selection," was, as is well known, introduced by Spencer.

Wallace's relations with Spencer, though somewhat controversial at times, were, nevertheless, cordial and sympathetic. In "My Life" he tells of his first visit, and the impression left upon his mind by their conversation. It occurred somewhere about

¹ "The Herbert Spencer Lecture," delivered at the Museum, December 8, 1910. (Clarendon Press, Oxford.)

1862-63, shortly after he and Bates had read, and been greatly impressed by, Spencer's "First Principles." "Our thoughts," he says, "were full of the great unsolved problem of the origin of life—a problem which Darwin's 'Origin of Species' left in as much obscurity as ever—and we looked to Spencer as the one man living who could give us some clue to it. His wonderful exposition of the fundamental laws and conditions, actions and interactions of the material universe seemed to penetrate so deeply into that 'nature of things' after which the early philosophers searched in vain . . . that we hoped he would throw some light on that great problem of problems. . . . He was very pleasant, spoke appreciatively of what we had both done for the practical exposition of evolution, and hoped we would continue to work at the subject. But when we touched upon the great problem, and whether he had arrived at even one of the first steps towards its solution, our hopes were dashed at once. That, he said, was too fundamental a problem to even think of solving at present. We did not yet know enough of matter in its essential constitution nor of the various forces of nature; and all he could say was that everything pointed to its having been a development out of matter—a phase of that continuous process of evolution by which the whole universe had been brought to its present condition. And so we had to wait and work contentedly at minor problems. And now, after forty years, though Spencer and Darwin and Weismann have thrown floods of light on the phenomena of life, its essential nature and its origin remain as great a mystery as ever. Whatever light we do possess is from a source which Spencer and Darwin neglected or ignored."¹

In his presidential address to the Entomological Society in 1872 Wallace made some special allusion to Spencer's theory of the origin of instincts, and on receiving a copy of the Address Spencer wrote: "It is gratifying to me to find that your extended knowledge does not lead you to scepticism respecting the speculation of mine which you quote, but rather enables you to cite further facts in justification of it. Possibly your exposition will lead some of those, in whose lines of investigation the question lies, to give deliberate attention to it." A further proof of his confidence was shown by asking Wallace (in 1874) to look over

¹ "My Life," ii. 23-4.

the proofs of the first six chapters of his "Principles of Sociology" in order that he might have the benefit of his criticisms alike as naturalist, anthropologist, and traveller.

This brief reference to the illustrious group of men to whom we owe the foundations of this new epoch of evolutionary thought—and not the foundations only, but also the patient building up of the structure upon which each one continued to perform his allotted task—and the prefatory notes and footnotes attached to the letters will serve to elucidate the historical correspondence between Darwin and Wallace which follows.

PART II.—(*Continued*)

II.—The Complete Extant Correspondence between Alfred Russel Wallace and Charles Darwin

[1857-1881]

"I hope it is a satisfaction to you to reflect—and very few things in my life have been more satisfactory to me—that we have never felt any jealousy towards each other, though in some senses rivals. I believe I can say this of myself with truth, and I am absolutely sure that it is true of you."—DARWIN to Wallace.

"To have thus inspired and retained this friendly feeling, notwithstanding our many differences of opinion, I feel to be one of the greatest honours of my life."—WALLACE to Darwin.

"I think the way he [Wallace] carries on controversy is perfectly beautiful, and in future histories of science the Wallace-Darwin episode will form one of the few bright points among rival claimants."—ERASMUS DARWIN to his niece, Henrietta Darwin, 1871.

THE first eight letters from Darwin to Wallace were found amongst the latter's papers, carefully preserved in an envelope on the outside of which he had written the words reproduced on the next page. Neither Wallace's part of this correspondence, nor the original MS. of his essay "On the Tendency of Varieties to Depart Indefinitely from the Original Type," which he sent to Darwin from Ternate, has been discovered. But these eight letters from Darwin explain themselves and reveal the inner story of the independent discovery of the theory of Natural Selection.

With respect to the letters which follow the first eight, both sides of the correspondence, with few exceptions, have been brought together. Some of the letters have already appeared in "The Life and Letters of Charles Darwin" and "More Letters," others in "My Life," by A. R. Wallace, whilst many have not before been published.

The first 8 letters I
received from Darwin -
while in the Malay Archipelago

NB. The Mss. of my Paper sent to
Darwin and printed in the Journal
of the Linnean Society, was not
returned to me, and seems to be
lost. The proofs with the Mss. were
perhaps sent to Sir Charles Lyell,
'or to the Secretary of the Linn. Soc.
& may some day be found. It
was written on thin foreign
note paper.

Alfred R. Wallace

FACSIMILE OF INSCRIPTION BY WALLACE ON THE ENVELOPE
IN WHICH HE KEPT THE FIRST EIGHT LETTERS HE RE-
CEIVED FROM DARWIN.

Some of these letters, in themselves, have little more than ephemeral interest, and parts of other letters could have been eliminated from the point of view of lightening this volume and of economising the reader's attention. But I decided, with the fullest approval of the Wallace and Darwin families, that the letters of these illustrious correspondents should be here presented as a whole, without mutilation.

Many of the notes of explanation to the Wallace letters have been gathered from his own writings, and are mainly in his own words; in such cases, the reader has the advantage of perusing letters annotated by their author, whilst many of the notes to the Darwin letters are by Sir F. Darwin.

LETTER I

C. DARWIN TO A. R. WALLACE

Down, Bromley, Kent. May 1, 1857.

My dear Sir,—I am much obliged for your letter of Oct. 10th from Celebes, received a few days ago: in a laborious undertaking, sympathy is a valuable and real encouragement. By your letter and even still more by your paper in the *Annals*,¹ a year or more ago, I can plainly see that we have thought much alike and to a certain extent have come to similar conclusions. In regard to the paper in the *Annals*, I agree to the truth of almost every word of your paper; and I daresay that you will agree with me that it is very rare to find oneself agreeing pretty closely with any theoretical paper; for it is lamentable how each man draws his own different conclusions from the very same fact. This summer will make the twentieth year (!) since I opened my first note-book on the question how and in what way do species and varieties differ from each other. I am now preparing my work for publication, but I find the subject so very large, that though I have written many chapters, I do not suppose I shall go to press for two years.

I have never heard how long you intend staying in the Malay

¹ "On the Law which has regulated the Introduction of New Species."—*Ann. and Mag. of Nat. Hist.*, 1855. The law is thus stated by Wallace: "Every species has come into existence co-incident both in time and space with a pre-existing closely-allied species."

Archipelago; I wish I might profit by the publication of your Travels there before my work appears, for no doubt you will reap a large harvest of facts.

I have acted already in accordance with your advice of keeping domestic varieties, and those appearing in a state of nature, distinct; but I have sometimes doubted of the wisdom of this, and therefore I am glad to be backed by your opinion. I must confess, however, I rather doubt the truth of the now very prevalent doctrine of all our domestic animals having descended from several wild stocks; though I do not doubt that it is so in some cases. I think there is rather better evidence on the sterility of hybrid animals than you seem to admit: and in regard to plants, the collection of carefully recorded facts by Kölreuter and Gaertner (and Herbert) is *enormous*. I most entirely agree with you on the little effect of "climatic conditions" which one sees referred to *ad nauseam* in all books: I suppose some very little effect must be attributed to such influences, but I fully believe that they are very slight. It is really *impossible* to explain my views in the compass of a letter as to causes and means of variation in a state of nature; but I have slowly adopted a distinct and tangible idea—whether true or false others must judge; for the firmest conviction of the truth of a doctrine by its author seems, alas, not to be the slightest guarantee of truth.

I have been rather disappointed at my results in the poultry line; but if you should, after receiving this, stumble on any curious domestic breed, I should be very glad to have it; but I can plainly see that the result will not be at all worth the trouble which I have taken. The case is different with the domestic pigeons; from its study I have learned much. The Rajah has sent me some of his pigeons and fowls and *cats'* skins from the interior of Borneo and from Singapore. Can you tell me positively that black jaguars or leopards are believed generally or always to pair with black? I do not think colour of offspring good evidence. Is the case of parrots fed on fat of fish turning colour mentioned in your Travels? I remember a case of parrots with (I think) poison from some toad put into hollow whence primaries had been removed.

One of the subjects on which I have been experimenting, and which cost me much trouble, is the means of distribution of all

organic beings found on oceanic islands; and any facts on this subject would be most gratefully received.

Land-molluscs are a great perplexity to me. This is a very dull letter, but I am a good deal out of health, and am writing this, not from my home, as dated, but from a water-cure establishment.

With most sincere good wishes for your success in every way, I remain, my dear Sir, yours sincerely,

CH. DARWIN.

LETTER II

C. DARWIN TO A. R. WALLACE

Down, Bromley, Kent. December 22, 1857.

My dear Sir,—I thank you for your letter of Sept. 27th. I am extremely glad to hear that you are attending to distribution in accordance with theoretical ideas. I am a firm believer that without speculation there is no good and original observation. Few travellers have attended to such points as you are now at work on; and indeed the whole subject of distribution of animals is dreadfully behind that of plants. You say that you have been somewhat surprised at no notice having been taken of your paper in the *Annals*. I cannot say that I am; for so very few naturalists care for anything beyond the mere description of species. But you must not suppose that your paper has not been attended to: two very good men, Sir C. Lyell, and Mr. E. Blyth at Calcutta, specially called my attention to it. Though agreeing with you on your conclusions in the paper, I believe I go much further than you; but it is too long a subject to enter on my speculative notions. I have not yet seen your paper on distribution of animals in the Aru Islands: I shall read it with the *utmost* interest; for I think that the most interesting quarter of the whole globe in respect to distribution; and I have long been very imperfectly trying to collect data from the Malay Archipelago. I shall be quite prepared to subscribe to your doctrine of subsidence: indeed from the quite independent evidence of the coral reefs I coloured my original map in my Coral volumes colours of the Aru Islands as one of subsidence, but got frightened and left it uncoloured. But I can see that you are inclined to go *much* further than I am in regard to the former

connection of oceanic islands with continents. Ever since poor E. Forbes propounded this doctrine, it has been eagerly followed; and Hooker elaborately discusses the former connection of all the Antarctic islands and New Zealand and South America. About a year ago I discussed the subject much with Lyell and Hooker (for I shall have to treat of it) and wrote out my arguments in opposition; but you will be glad to hear that neither Lyell nor Hooker thought much of my arguments; nevertheless, for once in my life I dare withstand the almost preternatural sagacity of Lyell. You ask about land-shells on islands far distant from continents: Madeira has a few identical with those of Europe, and here the evidence is really good, as some of them are sub-fossil. In the Pacific islands there are cases of identity, which I cannot at present persuade myself to account for by introduction through man's agency; although Dr. Aug. Gould has conclusively shown that many land-shells have thus been distributed over the Pacific by man's agency. These cases of introduction are most plaguing. Have you not found it so in the Malay Archipelago? It has seemed to me, in the lists of mammals of Timor and other islands, that *several* in all probability have been naturalised.

Since writing before, I have experimented a little on some land-molluscs, and have found sea-water not quite so deadly as I anticipated. You ask whether I shall discuss Man; I think I shall avoid the whole subject, as so surrounded with prejudices, though I fully admit that it is the highest and most interesting problem for the naturalist. My work, on which I have now been at work more or less for twenty years, will *not* fix or settle anything; but I hope it will aid by giving a large collection of facts with one definite end. I get on very slowly, partly from ill-health, partly from being a very slow worker. I have got about half written; but I do not suppose I shall publish under a couple of years. I have now been three whole months on one chapter on hybridism!

I am astonished to see that you expect to remain out three or four years more: what a wonderful deal you will have seen; and what an interesting area, the grand Malay Archipelago and the richest parts of South America! I infinitely admire and honour your zeal and courage in the good cause of natural science; and you have my very sincere and cordial good wishes

for success of all kinds; and may all your theories succeed, except that on oceanic islands, on which subject I will do battle to the death.—Pray believe me, my dear Sir, yours very sincerely,
C. DARWIN.

LETTER III

C. DARWIN TO A. R. WALLACE

Down, Bromley, Kent. January 25, 1859.

My dear Sir,—I was extremely much pleased at receiving three days ago your letter to me and that to Dr. Hooker. Permit me to say how heartily I admire the spirit in which they are written. Though I had absolutely nothing whatever to do in leading Lyell and Hooker to what they thought a fair course of action, yet I naturally could not but feel anxious to hear what your impression would be. I owe indirectly much to you and them; for I almost think that Lyell would have proved right and I should never have completed my larger work, for I have found my abstract¹ hard enough with my poor health; but now, thank God, I am in my last chapter but one. My abstract will make a small volume of 400 or 500 pages. Whenever published, I will of course send you a copy, and then you will see what I mean about the part which I believe selection has played with domestic productions. It is a very different part, as you suppose, from that played by “natural selection.”

I sent off, by same address as this note, a copy of the *Journal of the Linnean Society*, and subsequently I have sent some half-dozen copies of the Paper. I have many other copies at your disposal; and I sent two to your friend Dr. Davies (?), author of works on men's skulls.

I am glad to hear that you have been attending to birds' nests; I have done so, though almost exclusively under one point of view, viz. to show that instincts vary, so that selection could work on and improve them. Few other instincts, so to speak, can be preserved in a museum.

Many thanks for your offer to look after horses' stripes; if there are any donkeys, pray add them.

I am delighted to hear that you have collected bees' combs;

¹ “The Origin of Species.”

when next in London I will inquire of F. Smith and Mr. Saunders. This is an especial hobby of mine, and I think I can throw light on the subject. If you can collect duplicates at no very great expense, I should be glad of specimens for myself, with some bees of each kind. Young growing and irregular combs, and those which have not had pupæ, are most valuable for measurements and examination; their edges should be well protected against abrasion.

Everyone whom I have seen has thought your paper very well written and interesting. It puts my extracts (written in 1839, now just twenty years ago!), which I must say in apology were never for an instant intended for publication, in the shade.

You ask about Lyell's frame of mind. I think he is somewhat staggered, but does not give in, and speaks with horror often to me of what a thing it would be and what a job it would be for the next edition of the Principles if he were "perverted." But he is most candid and honest, and I think will end by being perverted. Dr. Hooker has become almost as heterodox as you or I—and I look at Hooker as *by far* the most capable judge in Europe.

Most cordially do I wish you health and entire success in all your pursuits; and God knows, if admirable zeal and energy deserve success, most amply do you deserve it. I look at my own career as nearly run out; if I can publish my abstract, and perhaps my greater work on the same subject, I shall look at my course as done.—Believe me, my dear Sir, yours very sincerely,

C. DARWIN.

LETTER IV

C. DARWIN TO A. R. WALLACE

Down, Bromley, Kent. April 6, 1859.

My dear Mr. Wallace,—I this morning received your pleasant and friendly note of Nov. 30th. The first part of my MS.¹ is in Murray's hands, to see if he likes to publish it. There is no preface, but a short Introduction, which must be read by everyone who reads my book. The second paragraph in the Introduction² I have had copied *verbatim* from my foul copy, and you will, I hope, think that I have fairly noticed your papers in the *Linnean*

¹ "The Origin of Species."

² First edit., 1859, pp. 1, 2.

Transactions.¹ You must remember that I am now publishing only an Abstract, and I give no references. I shall of course allude to your paper on Distribution;² and I have added that I know from correspondence that your explanation of your law is the same as that which I offer. You are right, that I came to the conclusion that Selection was the principle of change from study of domesticated productions; and then reading Malthus I saw at once how to apply this principle. Geographical distribution and geographical relations of extinct to recent inhabitants of South America first led me to the subject. Especially the case of the Galapagos Islands.

I hope to go to press in early part of next month. It will be a small volume of about 500 pages or so. I will, of course, send you a copy.

I forgot whether I told you that Hooker, who is our best British botanist, and perhaps the best in the world, is a *full* convert, and is now going immediately to publish his confession of faith; and I expect daily to see the proof-sheets. Huxley is changed and believes in mutation of species; whether a *convert* to us, I do not quite know. We shall live to see all the *younger* men converts. My neighbour and excellent naturalist, J. Lubbock, is an enthusiastic convert. I see by Natural History notices that you are doing great work in the Archipelago; and most heartily do I sympathise with you. For God's sake take care of your health. There have been few such noble labourers in the cause of natural science as you are. Farewell, with every good wish.—Yours sincerely,

C. DARWIN.

P.S.—You cannot tell how I admire your spirit, in the manner in which you have taken all that was done about publishing our papers. I had actually written a letter to you, stating that I would *not* publish anything before you had published. I had not sent that letter to the post when I received one from Lyell and

¹ "On the Tendency of Species to form Varieties and on the Perpetuation of Varieties and Species by Natural Means of Selection." By C. Darwin and A. R. Wallace. Communicated by Sir C. Lyell and J. D. Hooker. *Journ. Linn. Soc.*, iii. 45, 1859. Read July 1st, 1858.

² "On the Law which has regulated the Introduction of New Species." *Ann. and Mag. of Nat. Hist.*, 1855, xvi. 184.

Hooker, *urging* me to send some MS. to them, and allow them to act as they thought fair and honourably to both of us. I did so.

LETTER V

C. DARWIN TO A. R. WALLACE

Down, Bromley, Kent. August 9, 1859.

My dear Mr. Wallace,—I received your letter and memoir¹ on the 7th, and will forward it to-morrow to the Linnean Society. But you will be aware that there is no meeting till beginning of November. Your paper seems to me *admirable* in matter, style and reasoning; and I thank you for allowing me to read it. Had I read it some months ago I should have profited by it for my forthcoming volume. But my two chapters on this subject are in type; and though not yet corrected, I am so wearied out and weak in health that I am fully resolved not to add one word, and merely improve style. So you will see that my views are nearly the same with yours, and you may rely on it that not one word shall be altered owing to my having read your ideas. Are you aware that Mr. W. Earl published several years ago the view of distribution of animals in the Malay Archipelago in relation to the depth of the sea between the islands? I was much struck with this, and have been in habit of noting all facts on distribution in the Archipelago and elsewhere in this relation. I have been led to conclude that there has been a good deal of naturalisation in the different Malay islands, and which I have thought to certain extent would account for anomalies. Timor has been my greatest puzzle. What do you say to the peculiar *Felis* there? I wish that you had visited Timor: it has been asserted that a fossil mastodon or elephant's tooth (I forget which) had been found there, which would be a grand fact. I was aware that Celebes was very peculiar; but the relation to Africa is quite new to me and marvellous and almost passes belief. It is as anomalous as the relation of plants in South-West Australia to the Cape of Good Hope.

I differ *wholly* from you on colonisation of *oceanic* islands, but you will have *everyone* else on your side. I quite agree with respect

¹ This seems to refer to Wallace's paper on "The Zoological Geography of the Malay Archipelago," *Linn. Soc. Journ.*, 1860.

to all islands not situated far in ocean. I quite agree on little occasional internavigation between lands when once pretty well stocked with inhabitants, but think this does not apply to rising and ill-stocked islands.

Are you aware that *annually* birds are blown to Madeira, to Azores (and to Bermuda from America). I wish I had given fuller abstract of my reasons for not believing in Forbes's great continental extensions; but it is too late, for I will alter nothing. I am worn out, and must have rest.

Owen, I do not doubt, will bitterly oppose us; but I regard that very little, as he is a poor reasoner and deeply considers the good opinion of the world, especially the aristocratic world.

Hooker is publishing a grand Introduction to the Flora of Australia, and goes the whole length. I have seen proofs of about half.—With every good wish, believe me, yours very sincerely,
C. DARWIN.

Excuse this brief note, but I am far from well.

LETTER VI

C. DARWIN TO A. R. WALLACE

Ilkley. November 13, 1859.

My dear Sir,—I have told Murray to send you by post (if possible) a copy of my book, and I hope that you will receive it at nearly the same time with this note. (N.B.—I have got a bad finger, which makes me write extra badly.) If you are so inclined, I should very much like to hear your general impression of the book, as you have thought so profoundly on the subject and in so nearly the same channel with myself. I hope there will be some little new to you, but I fear not much. Remember, it is only an abstract, and very much condensed. God knows what the public will think. No one has read it, except Lyell, with whom I have had much correspondence. Hooker thinks him a complete convert, but he does not seem so in his letters to me. But he is evidently deeply interested in the subject. I do not think your share in the theory will be overlooked by the real judges, as Hooker, Lyell, Asa Gray, etc.

I have heard from Mr. Sclater that your paper on the Malay

Archipelago has been read at the Linnean Society, and that he was *extremely* much interested by it.

I have not seen one naturalist for six or nine months owing to the state of my health, and therefore I really have no news to tell you. I am writing this at Ilkley Wells, where I have been with my family for the last six weeks, and shall stay for some few weeks longer. As yet I have profited very little. God knows when I shall have strength for my bigger book.

I sincerely hope that you keep your health: I suppose that you will be thinking of returning soon with your magnificent collection and still grander mental materials. You will be puzzled how to publish. The Royal Society Fund will be worth your consideration.—With every good wish, pray believe me, yours very sincerely,

CHARLES DARWIN.

I think I told you before that Hooker is a complete convert. If I can convert Huxley I shall be content.

LETTER VII

C. DARWIN TO A. R. WALLACE

Down, Bromley, Kent, S.E. March 7, 1860.

My dear Wallace,—The addresses which you have sent me are capital, especially that to the Rajah; and I have dispatched two sets of queries. I now enclose a copy to you, and should be very glad of any answers; you must not suppose the P.S. about memory has lately been inserted; please return these queries, as it is my standard copy. The subject is a curious one; I fancy I shall make a rather interesting appendix to my Essay on Man.

I fully admit the probability of "protective adaptation" having come into play with female butterflies as well as with female birds. I have a good many facts which make me believe in sexual selection as applied to man, but whether I shall convince anyone else is very doubtful.—Dear Wallace, yours very sincerely,

CH. DARWIN.

LETTER VIII

C. DARWIN TO A. R. WALLACE

Down, Bromley, Kent. May 18, 1860.

My dear Mr. Wallace,—I received this morning your letter from Amboyna dated Feb. 16th, containing some remarks and your too high approbation of my book. Your letter has pleased me very much, and I most completely agree with you on the parts which are strongest and which are weakest. The imperfection of the geological record is, as you say, the weakest of all; but yet I am pleased to find that there are almost more geological converts than of pursuers of other branches of natural science. I may mention Lyell, Ramsay, Jukes, Rogers, Keyerling, all good men and true. Pictet of Geneva is not a convert, but is evidently staggered (as I think is Bronn of Heidelberg), and he has written a perfectly fair review in the *Bib. Universelle* of Geneva. Old Bronn has translated my book, well done also into German, and his well-known name will give it circulation. I think geologists are more converted than simple naturalists because more accustomed to reasoning.

Before telling you about the progress of opinion on the subject, you must let me say how I admire the generous manner in which you speak of my book: most persons would in your position have felt bitter envy and jealousy. How nobly free you seem to be of this common failing of mankind. But you speak far too modestly of yourself; you would, if you had had my leisure, have done the work just as well, perhaps better, than I have done it. Talking of envy, you never read anything more envious and spiteful (with numerous misrepresentations) than Owen is in the *Edinburgh Review*. I must give one instance; he throws doubts and sneers at my saying that the ovigerous frena of cirripedes have been converted into branchiæ, because I have not found them to be branchiæ; whereas *he himself* admits, before I wrote on cirripedes, without the least hesitation, that their organs are branchiæ. The attacks have been heavy and incessant of late. Sedgwick and Prof. Clarke attacked me savagely at the Cambridge Philosophical Society, but Henslow defended me well, though not a convert. Phillips has since attacked me in a lecture at Cambridge. Sir W. Jardine in the *Edinburgh New*

Philosophical Journal, Wollaston in the *Annals of Nat. History*, A. Murray before the Royal Soc. of Edinburgh, Haughton at the Geological Society of Dublin, Dawson in the *Canadian Nat. Magazine*, and many others. But I am getting case-hardened, and all these attacks will make me only more determinedly fight. Agassiz sends me personal civil messages, but incessantly attacks me; but Asa Gray fights like a hero in defence. Lyell keeps as firm as a tower, and this autumn will publish on the Geological History of Man, and will then declare his conversion, which now is universally known. I hope that you have received Hooker's splendid essay. So far is bigotry carried that I can name three botanists who will not even read Hooker's essay!! Here is a curious thing: a Mr. Pat. Matthew, a Scotchman, published in 1830 a work on Naval Timber and Arboriculture, and in the appendix to this he gives *most clearly* but very briefly in half-dozen paragraphs our view of Natural Selection. It is a most complete case of anticipation. He published extracts in the *Gardeners' Chronicle*. I got the book, and have since published a letter acknowledging that I am fairly forestalled. Yesterday I heard from Lyell that a German, Dr. Schaffhausen, has sent him a pamphlet published some years ago, in which the same view is nearly anticipated, but I have not yet seen this pamphlet. My brother, who is a very sagacious man, always said, "You will find that someone will have been before you." I am at work at my larger work, which I shall publish in separate volumes. But for ill health and swarms of letters I get on very, very slowly. I hope that I shall not have wearied you with these details.

With sincere thanks for your letter, and with most deeply-felt wishes for your success in science and in every way, believe me, your sincere well-wisher,

C. DARWIN.

Of the letters from Wallace to Darwin which have been preserved, the earliest is the following:

5 Westbourne Grove Terrace, W. April 7, 1862.

My dear Mr. Darwin,—I was much pleased to receive your note this morning. I have not yet begun work, but hope to be soon busy. As I am being doctored a little I do not think I shall be

able to accept your kind invitation at present, but trust to be able to do so during the summer.

I beg you to accept a wild honeycomb from the island of Timor, not quite perfect but the best I could get. It is of a small size, but of characteristic form, and I think will be interesting to you. I was quite unable to get the honey out of it, so fear you will find it somewhat in a mess; but no doubt you will know how to clean it. I have told Stevens to send it to you.

Hoping your health is now quite restored and with best wishes, I remain, my dear Mr. Darwin, yours very sincerely,

ALFRED R. WALLACE.

5 Westbourne Grove Terrace, W. May 23, 1862.

My dear Mr. Darwin,—Many thanks for your most interesting book on the Orchids. I have read it through most attentively, and have really been quite as much staggered by the wonderful adaptations you show to exist in them as by the *Eye* in animals or any other implicated organs. I long to get into the country and have a look at some orchids guided by your new lights, but I have been now for ten days confined to my room with what is disagreeable though far from dangerous—boils.

I have been reading several of the Reviews on the "Origin," and it seems to me that you have assisted those who want to criticise you by your overstating the difficulties and objections. Several of them quote your own words as the strongest arguments against you.

I think you told me Owen wrote the article in the *Quarterly*. This seems to me hardly credible, as he speaks so much of Owen, quotes him as such a great authority, and I believe even calls him a profound philosopher, etc., etc. Would Owen thus speak of himself?

Trusting your health is good, I remain, my dear Mr. Darwin, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. May 24, 1862.

My dear Mr. Wallace,—I write one line to thank you for your note and to say that the Bishop of Oxford¹ wrote the *Quarterly*

¹ Bishop Samuel Wilberforce.

Review (paid £60), aided by Owen. In the *Edinburgh* Owen no doubt praised himself. Mr. Maw's *Review* in the *Zoologist* is one of the best, and staggered me in parts, for I did not see the sophistry of parts. I could lend you any which you might wish to see; but you would soon be tired. Hopkins in France and Pictet are two of the best.

I am glad you approve of my little Orchid book; but it has not been worth, I fear, the ten months it has cost me: it was a hobby horse, and so beguiled me.

I am sorry to hear that you are suffering from boils; I have often had fearful crops: I hope that the doctors are right in saying that they are serviceable.

How puzzled you must be to know what to begin at. You will do grand work, I do not doubt.

My health is, and always will be, very poor: I am that miserable animal a regular valetudinarian.—Yours very sincerely,

C. DARWIN.

5 Westbourne Grove Terrace, W. August 8, 1862.

My dear Mr. Darwin,—I sincerely trust that your little boy is by this time convalescent, and that you are therefore enabled to follow your favourite investigations with a more tranquil mind.

I heard a remark the other day which may not perhaps be new to you, but seemed to me a fact if true, in your favour. Mr. Ward (I think it was), a member of the Microscopical Society, mentioned as a fact noticed by himself with much surprise that "the muscular fibres of the whale were no larger than those of the bee!—an excellent indication of community of origin.

While looking at the ostriches the other day at the Gardens, it occurred to me that they were a case of special difficulty, as, inhabiting an ancient continent, surrounded by numerous enemies, how did their wings ever become abortive, and if they did so before the birds had attained their present gigantic size, strength and speed, how could they in the transition have maintained their existence? I see Westwood in the *Annals* brings forward the same case, arguing that the ostriches should have acquired better wings within the historic period; but as they are now the swiftest of animals they evidently do not want their wings, which in their present state may serve some other trifling

purpose in their economy such as fans, or balancers, which may have prevented their being reduced to such rudiments as in the cassowaries. The difficulty to me seems to be, how, if they once had flight, could they have lost it, surrounded by swift and powerful carnivora against whom it must have been the only defence.

This probably is all clear to you, but I think it is a point you might touch upon, as I think the objection will seem a strong one to most people.

In a day or two I go to Devonshire for a few weeks and hope to lay in a stock of health to enable me to stick to work at my collections during the winter. I begin to find that large collections involve a heavy amount of manual labor which is not very agreeable.

Present my compliments to Mrs. and Miss Darwin and believe me, yours very faithfully,

ALFRED R. WALLACE.

1 Carlton Terrace, Southampton. August 20, 1862.

My dear Mr. Wallace,—You will not be surprised that I have been slow in answering when I tell you that my poor (boy)¹ became frightfully worse after you were at Down; and that during our journey to Bournemouth he had a slight relapse here and my wife took the scarlet fever rather severely. She is over the crisis. I have had a horrid time of it, and God only knows when we shall be all safe at home again—half my family are at Bournemouth.

I have given a piece of the comb from Timor to a Mr. Woodbury (who is working at the subject) and he is *extremely* interested by it (I was sure the specimen would be valuable), and has requested me to ascertain whether the bee (*A. testacea*) is domesticated when it makes its combs? Will you kindly inform me?

Your remarks on ostriches have interested me, and I have alluded to the case in the Third Edition. The difficulty does not seem to be so great as to you. Think of bustards, which inhabit wide open plains, and which so seldom take flight: a very little increase in size of body would make them incapable of flight. The idea of ostriches acquiring flight is worthy of Westwood; think of the food required in these inhabitants of the desert

¹ Now Major Leonard Darwin.

to work the pectoral muscles! In the rhea the wings seem of considerable service in the first start and in turning.¹ . . .

5 Westbourne Grove Terrace, W. September 30, 1862.

My dear Mr. Darwin,—Many thanks for the third edition of the "Origin," which I found here on my return from Devonshire on Saturday. I have not had time yet to read more than the Historical Sketch, which is very interesting, and shows that the time had quite come for your book.

I am now reading Herbert Spencer's "First Principles," which seems to me a truly great work, which goes to the root of everything.

I hope you will be well enough to come to Cambridge.

I remain, my dear Mr. Darwin, yours very faithfully,

ALFRED R. WALLACE.

5 Westbourne Grove Terrace, W. January 14, [1863?].

My dear Mr. Darwin,—I am very sorry indeed to hear you are still in weak health. Have you ever tried mountain air? A residence at 2,000 or 3,000 ft. elevation is very invigorating.

I trust your family are now all in good health, and that you may be spared any anxiety on that score for some time. If you come to town I shall hope to have the pleasure of seeing you.

I am now in much better health, but find sudden changes of weather affect me very much, bringing on ague and fever fits. I am now working a little, but having fresh collections still arriving from correspondents in the East, it is principally the drudgery of cleaning, packing, and arrangement.

On the opposite page I give all the information I can about the Timor fossils, so that you can send it entire to Dr. Falconer.

With best wishes for the speedy recovery of your health, I remain, my dear Mr. Darwin, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. January 1, 1864.

Dear Wallace,—I am still unable to write otherwise than by dictation. In a letter received two or three weeks ago from Asa

¹ The last sheet of the letter is missing.

Gray he writes: "I read lately with gusto Wallace's exposé of the Dublin man on Bee cells, etc."¹

Now though I cannot read at present, I much want to know where this is published, that I may procure a copy. Further on Asa Gray says (after speaking of Agassiz's paper on Glaciers in the *Atlantic Magazine* and his recent book entitled "Method of Study"): "Pray set Wallace upon these articles." So Asa Gray seems to think much of your powers of reviewing, and I mention this as it assuredly is *laudari a laudato*.

I hope you are hard at work, and if you are inclined to tell me I should much like to know what you are doing.

It will be many months, I fear, before I shall do anything.

Pray believe me, yours very sincerely,

CH. DARWIN.

5 Westbourne Grove Terrace, W. January 2, 1864.

My dear Darwin,—Many thanks for your kind letter. I was afraid to write because I heard such sad accounts of your health, but I am glad to find that you can write, and I presume read, by deputy. My little article on Houghton's paper was published in the *Annals of Natural History* about August or September last, I think, but I have not a copy to refer to. I am sure it does not deserve Asa Gray's praises, for though the matter may be true enough, the manner I know is very inferior. It was written hastily, and when I read it in the *Annals* I was rather ashamed of it, as I knew so many could have done it so much better.

I will try and see Agassiz's paper and book. What I have hitherto seen of his on Glacial subjects seems very good, but in all his Natural History *theories*, he seems so utterly wrong and so totally blind to the plainest deduction from facts, and at the same time so vague and obscure in his language, that it would be a very long and wearisome task to answer him.

With regard to work, I am doing but little—I am afraid I have no good habit of systematic work. I have been gradually getting parts of my collections in order, but the obscurities of

¹ Wallace's paper was entitled "Remarks on the Rev. S. Houghton's Paper on the Bee's Cells and on the Origin of Species." Prof. Houghton's paper was read before the Natural History Society of Dublin, November 1st, 1862, and reprinted in *Ann. and Mag. of Nat. Hist.*, 1863, xi. 415.

synonymy and descriptions, the difficulty of examining specimens, and my very limited library, make it wearisome work.

I have been lately getting the first groups of my butterflies in order, and they offer some most interesting facts in variation and distribution—in variation some very puzzling ones. Though I have very fine series of specimens, I find in many cases I want more; in fact if I could have afforded to have all my collections kept till my return I should I think have found it necessary to retain twice as many as I now have.

I am at last making a beginning of a small book on my Eastern journey, which, if I can persevere, I hope to have ready by next Christmas. I am a very bad hand at writing anything like narrative. I want something to argue on, and then I find it much easier to go ahead. I rather despair, therefore, of making so good a book as Bates's, though I think my subject is better. Like every other traveller, I suppose, I feel dreadfully the want of copious notes on common everyday objects, sights and sounds and incidents, which I imagined I could never forget but which I now find it impossible to recall with any accuracy.

I have just had a long and most interesting letter from my old companion Spruce. He says he has had a letter from you about *Melastoma*, but has not, he says, for three years seen a single melastomaceous plant! They are totally absent from the Pacific plains of tropical America, though so abundant on the Eastern plains. Poor fellow, he seems to be in a worse state than you are. Life has been a burden to him for three years owing to lung and heart disease, and rheumatism, brought on by exposure in high, hot, and cold damp valleys of the Andes. He went down to the dry climate of the Pacific coast to die more at ease, but the change improved him, and he thinks to come home, though he is sure he will not survive the first winter in England. He had never been able to get a copy of your book, though I am sure no one would have enjoyed or appreciated it more.

If you are able to bear reading, will you allow me to take the liberty of recommending you a book? The fact is I have been so astonished and delighted with the perusal of Spencer's works that I think it a duty to society to recommend them to all my friends who I think can appreciate them. The one I particularly refer to now is "*Social Statics*," a book which is by no means hard to read; it is even amusing, and owing to the wonderful clearness

of its style may be read and understood by anyone. I think, therefore, as it is quite distinct from your special studies at present, you might consider it as "light literature," and I am pretty sure it would interest you more than a great deal of what is now considered very good. I am utterly astonished that so few people seem to read Spencer, and the utter ignorance there seems to be among politicians and political economists of the grand views and logical stability of his works. He appears to me as far ahead of John Stuart Mill as J. S. M. is of the rest of the world, and, I may add, as Darwin is of Agassiz. The range of his knowledge is no less than its accuracy. His nebular hypothesis in the last volume of his essays is the most masterly astronomical paper I have ever read, and in his forthcoming volume on Biology he is I understand going to show that there is something else besides Natural Selection at work in nature. So you must look out for a "foeman worthy of your steel"! But perhaps all this time you have read his books. If so, excuse me, and pray give me your opinion of him, as I have hitherto only met with one man (Huxley) who has read and appreciated him.

Allow me to say in conclusion how much I regret that unavoidable circumstances have caused me to see so little of you since my return home, and how earnestly I pray for the speedy restoration of your health.—Yours most sincerely,

ALFRED R. WALLACE.

Malvern Wells. Tuesday, March, 1864.

My dear Mr. Wallace,—Your kindness is neverfailing. I got worse and worse at home and was sick every day for two months; so came here, when I suddenly broke down and could do nothing; but I hope I am now very slowly recovering, but am very weak.

Sincere thanks about *Melastoma*: these flowers have baffled me, and I have caused several friends much useless labour; though, Heaven knows, I have thrown away time enough on them myself.

The gorse case is very valuable, and I will quote it, as I presume I may.

I was very glad to see in the *Reader* that you have been giving a grand paper (as I infer from remarks in discussion) on Geographical Distribution.

I am very weak, so will say no more.—Yours very sincerely,
C. DARWIN.

In Vol. I., p. 93, of the "Life and Letters of Charles Darwin," Darwin states the circumstances which led to his writing the "Descent of Man." He says that his collection of facts, begun in 1837 or 1838, was continued for many years without any definite idea of publishing on the subject. The following letter to Wallace of May 28, 1864, in reply to the latter's of May 10, shows that in the period of ill-health and depression about 1864 he despaired of ever being able to do so.

5 Westbourne Grove Terrace, W. May 10, 1864.

My dear Darwin,—I was very much gratified to hear by your letter of a month back that you were a little better, and I have since heard occasionally through Huxley and Lubbock that you are not worse. I sincerely hope the summer weather and repose may do you real good.

The Borneo Cave exploration is to go on at present without a subscription. The new British consul who is going out to Sarawak this month will undertake to explore some of the caves nearest the town, and if anything of interest is obtained a good large sum can no doubt be raised for a thorough exploration of the whole country. Sir J. Brooke will give every assistance, and will supply men for the preliminary work.

I send you now my little contribution to the *theory* of the origin of man. I hope you will be able to agree with me. If you are able, I shall be glad to have your criticisms.

I was led to the subject by the necessity of explaining the vast mental and cranial differences between man and the apes combined with such small structural differences in other parts of the body, and also by an endeavour to account for the diversity of human races combined with man's almost perfect stability of form during all historical epochs.

It has given me a settled opinion on these subjects, if nobody can show a fallacy in the argument.

The Anthropologicals did not seem to appreciate it much, but we had a long discussion which appears almost verbatim in the *Anthropological Review*.¹

¹ For March, 1864.

As the *Linnean Transactions* will not be out till the end of the year I sent a pretty full abstract of the more interesting parts of my Papilionidæ paper¹ to the *Reader*, which, as you say, is a splendid paper.

Trusting Mrs. Darwin and all your family are well, and that you are improving, believe me, yours most sincerely,

ALFRED R. WALLACE.

Down, Bromley, Kent. May 28, 1864.

Dear Wallace,—I am so much better that I have just finished a paper for the Linnean Society; but as I am not yet at all strong I felt much disinclination to write, and therefore you must forgive me for not having sooner thanked you for your paper on Man received on the 11th. But first let me say that I have hardly ever in my life been more struck by any paper than that on variation, etc., etc., in the *Reader*. I feel sure that such papers will do more for the spreading of our views on the modification of species than any separate treatises on the single subject itself. It is really admirable; but you ought not in the Man paper to speak of the theory as mine; it is just as much yours as mine. One correspondent has already noticed to me your "high-minded" conduct on this head.

But now for your Man paper, about which I should like to write more than I can. The great leading idea is quite new to me, viz. that during late ages the mind will have been modified more than the body; yet I had got as far as to see with you that the struggle between the races of man depended entirely on intellectual and *moral* qualities. The latter part of the paper I can designate only as grand and most eloquently done. I have shown your paper to two or three persons who have been here, and they have been equally struck with it.

I am not sure that I go with you on all minor points. When reading Sir G. Grey's account of the constant battles of Australian savages, I remember thinking that Natural Selection would come in, and likewise with the Esquimaux, with whom the art of fishing and managing canoes is said to be hereditary. I rather differ on the rank under the classificatory point of view

¹ *Reader*, April 16, 1864. An abstract of Wallace's paper "On the Phenomena of Variation and Geographical Distribution, as illustrated by the Papilionidæ of the Malayan Region." *Linn. Soc. Trans.*, xxv.

which you assign to Man: I do not think any character simply in excess ought ever to be used for the higher division. Ants would not be separated from other hymenopterous insects, however high the instinct of the one and however low the instincts of the other.

With respect to the differences of race, a conjecture has occurred to me that much may be due to the correlation of complexion (and consequently hair) with constitution. Assume that a dusky individual best escaped miasma and you will readily see what I mean. I persuaded the Director-General of the Medical Department of the Army to send printed forms to the surgeons of all regiments in tropical countries to ascertain this point, but I daresay I shall never get any returns. Secondly, I suspect that a sort of sexual selection has been the most powerful means of changing the races of man. I can show that the different races have a widely different standard of beauty. Among savages the most powerful men will have the pick of the women, and they will generally leave the most descendants.

I have collected a few notes on Man, but I do not suppose I shall ever use them. Do you intend to follow out your views, and if so would you like at some future time to have my few references and notes?

I am sure I hardly know whether they are of any value, and they are at present in a state of chaos.

There is much more that I should like to write but I have not strength.—Believe me, dear Wallace, yours very sincerely,

CH. DARWIN.

Our aristocracy is handsomer? (more hideous according to a Chinese or negro) than middle classes, from pick of women; but oh what a scheme is primogeniture for destroying Natural Selection. I fear my letter will be barely intelligible to you.

5 Westbourne Grove Terrace, W. May 29, [1864].

My dear Darwin,—You are always so ready to appreciate what others do, and especially to overestimate my desultory efforts, that I cannot be surprised at your very kind and flattering remarks on my papers. I am glad, however, that you have made a few critical observations, and am only sorry you were not well

enough to make more, as that enables me to say a few words in explanation.

My great fault is haste. An idea strikes me, I think over it for a few days, and then write away with such illustrations as occur to me while going on. I therefore look at the subject almost solely from one point of view. Thus in my paper on Man¹ I aim solely at showing that brutes are modified in a *great variety* of ways by Natural Selection, but that in *none of these particular* ways can man be modified, because of the superiority of his intellect. I therefore no doubt overlook a few smaller points in which Natural Selection may still act on men and brutes alike. Colour is one of them, and I have alluded to this in correlation to constitution, in an abstract I have made at Sclater's request for the *Natural History Review*.² At the same time, there is so much evidence of migrations and displacements of races of man, and so many cases of peoples of distinct physical characters inhabiting the same or similar regions, and also of races of uniform physical characters inhabiting widely dissimilar regions, that the external characteristics of the chief races of man must I think be older than his present geographical distribution, and the modifications produced by correlation to favourable variations of constitution be only a secondary cause of external modification.

I hope you may get the returns from the Army. They would be very interesting, but I do not expect the results would be favourable to your view.

With regard to the constant battles of savages leading to selection of physical superiority, I think it would be very imperfect, and subject to so many exceptions and irregularities, that it could produce no *definite* result. For instance, the strongest and bravest men would lead, and expose themselves most, and would therefore be most subject to wounds and death. And the physical energy which led to any one tribe delighting in war might lead to its extermination by inducing quarrels with all surrounding tribes and leading them to combine against it. Again, superior cunning, stealth and swiftness of foot, or even better weapons, would often lead to victory as well as mere physical strength. Moreover this kind of more or less perpetual war

¹ *Anthropolog. Rev.*, 1864.

² *Nat. Hist. Rev.*, 1864, p. 328.

goes on among all savage peoples. It could lead therefore to no differential characters, but merely to the keeping up of a certain average standard of bodily and mental health and vigour. So with selection of variations adapted to special habits of life, as fishing, paddling, riding, climbing, etc., etc., in different races: no doubt it must act to some extent, but will it be ever so rigid as to induce a definite physical modification, and can we imagine it to have had any part in producing the distinct races that now exist?

The sexual selection you allude to will also, I think, have been equally uncertain in its results. In the very lowest tribes there is rarely much polygamy, and women are more or less a matter of purchase. There is also little difference of social condition, and I think it rarely happens that any healthy and undeformed man remains without wife and children. I very much doubt the often-repeated assertion that our aristocracy are more beautiful than the middle classes. I allow that they present *specimens* of the highest kind of beauty, but I doubt the average. I have noticed in country places a greater average amount of good looks among the middle classes, and besides, we unavoidably combine in our idea of beauty, intellectual expression and refinement of *manner*, which often make the less appear the more beautiful. Mere physical beauty—that is, a healthy and regular development of the body and features approaching to the *mean* or *type* of European man—I believe is quite as frequent in one class of society as the other, and much more frequent in rural districts than in cities.

With regard to the rank of man in zoological classification, I fear I have not made myself intelligible. I never meant to adopt Owen's or any other such views, but only to point out that from *one* point of view he was right. I hold that a distinct *family* for man, as Huxley allows, is all that can possibly be given him zoologically. But at the same time, if my theory is true—that while the animals which surrounded him have been undergoing modification in *all* parts of their bodies to a *generic* or even *family* degree of difference, he has been changing almost wholly in the brain and head—then, in geological antiquity the *species* of man may be as old as many mammalian *families*, and the origin of the *family* man may date back to a period when some of the orders first originated.

As to the theory of Natural Selection itself, I shall always maintain it to be actually yours and yours only. You had worked it out in details I had never thought of, years before I had a ray of light on the subject, and my paper would never have convinced anybody or been noticed as more than an ingenious speculation, whereas your book has revolutionised the study of natural history, and carried away captive the best men of the present age. All the merit I claim is the having been the means of inducing *you* to write and publish at once.

I may possibly some day go a little more into this subject (of Man), and, if I do, will accept the kind offer of your notes. I am now, however, beginning to write the "Narrative of my Travels" which will occupy me a long time, as I hate writing narrative, and after Bates's brilliant success rather fear to fail. I shall introduce a few chapters on geographical distribution and other such topics.

Sir C. Lyell, while agreeing with my main argument on Man, thinks I am wrong in wanting to put him back into Miocene times, and thinks I do not appreciate the immense interval even to the later Pliocene. But I still maintain my view, which in fact is a logical result of my theory, for if man originated in later Pliocene times, when almost all mammalia were of closely allied species to those now living, and many even identical, then man has *not* been stationary in bodily structure while animals have been varying, and my theory will be proved to be all wrong.

In Murchison's address to the Geographical Society just delivered he points out Africa as being the *oldest* existing land. He says there is *no* evidence of its having been ever submerged during the tertiary epoch. Here, then, is evidently the place to find *early man*. I hope something good may be found in Borneo and that then means may be found to explore the still more promising regions of tropical Africa, for we can expect nothing of man *very* early in Europe.

It has given me great pleasure to find that there are symptoms of improvement in your health. I hope you will not exert yourself too soon or write more than is quite agreeable to you. I think I made out every word of your letter though it was not always easy.—Believe me, my dear Darwin, yours very sincerely,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. June 15, 1864.

Dear Wallace,—You must not suppose from my delay that I have not been much interested by your long letter. I write now merely to thank you, and just to say that probably you are right on all the points you touch on except, as I think, about sexual selection, which I will not give up.

My belief in it, however, is contingent on my general beliefs in sexual selection. It is an awful stretch to believe that a peacock's tail was thus formed; but believing it, I believe in the same principle somewhat modified applied to man.

I doubt whether my notes would be of any use to you, and as far as I remember they are chiefly on sexual selection.

I am very glad to hear that you are on your Travels. I believe you will find it a very convenient vehicle for miscellaneous discussion. With your admirable powers of writing, I cannot doubt that you will make an excellent book.—Believe me, dear Wallace, yours sincerely,

CH. DARWIN.

P.S.—A great gun, Flourens, has written a little dull book against me; which pleases me much, for it is plain that our good work is spreading in France. He speaks of the *engouement* about this book, "so full of empty and presumptuous thoughts."

Down, Bromley, Kent, S.E. January 29, 1865.

My dear Wallace,—I must ease my mind by saying how much I admire the two papers you have sent me.

That on parrots¹ contained most new matter to me, and interested me *extremely*; that in the *Geographical Journal*² strikes me as an epitome of the whole theory of geographical distribution: the comparison of Borneo and New Guinea, the relation of the volcanic outbursts and the required subsidence, and the comparison of the supposed conversion of the Atlantic into a great archipelago, seemed to me the three best hits. They are both indeed excellent papers.—Believe me, yours very sincerely,

CHARLES DARWIN.

· Do try what hard work will do to banish painful thoughts.³

¹ "Read June, 1864."—A. R. W. ² "June 8, 1864" (?).—A. R. W.

³ "Referring to my broken engagement."—A. R. W.

P.S.—During one of the later French voyages, a *wild* pig was killed and brought from the Aru Islands to Paris. Am I not right in inferring that this must have been introduced and run wild? If you have a clear opinion on this head, may I quote you?

5 Westbourne Grove Terrace, W. January 31 [1865?].

Dear Darwin,—Many thanks for your kind letter. I send you now a few more papers. One on Man is not much in your line. The other three are bird lists, but in the introductory remarks are a few facts of distribution that may be of use to you, and as you have them already in the *Zoological Proceedings*, you can cut these up if you want “extracts.”

I hope you do not very much want the Aru pig to be a domestic animal run wild, because I have no doubt myself it was the species peculiar to the New Guinea fauna (*Sus papuensis*, Less.), a very distinct form. I have no doubt it is this species, though I did not get it myself there, because I was told that on a small island near, called there Pulo babi (Pig Island), was a race of pigs (different from and larger than those of the large islands) which had originated from the wreck of a large ship near a century ago. The productions of the Aru Islands closely resemble those of New Guinea, more than half the species of birds being identical, as well as about half of the few known mammals.

I am beginning to work at some semi-mechanical work, drawing up catalogues of parts of my collection for publication.

I enclose my “carte.” Have you a photograph of yourself of any kind you can send me? When you come to town next, may I beg the honour of a sitting for my brother-in-law, Mr. Sims, 73 Westbourne Grove?—Yours very sincerely,

ALFRED R. WALLACE.

P.S.—Your paper on *Lythrum salicaria*¹ is most beautiful.

What a wonderful plant it is! I long to hear your paper on Tendrils and hear what you have got out of them. My old friend Spruce, a good botanist and close observer, could probably supply you with some facts on that or other botanical subjects if you would write to him. He is now at Kew, but almost as ill as yourself.—A. R. W.

¹ Paper on the three forms of *Lythrum*.

Down, Bromley, Kent, S.E. February 1, 1865.

My dear Wallace,—I am much obliged for your photograph, for I have lately set up a scientific album; and for the papers, which I will read before long. I enclose my own photo, taken by my son, and I have no other.

I fear it will be a long time before I shall be able to sit to a photographer, otherwise I should be happy to sit to Mr. Sims.

Thanks for information about the Aru pig, which will make me very cautious.

It is a perplexing case, for Nathusius says the skull of the Aru resembles that of the Chinese breed, and he thinks that *Sus papuensis* has been founded on a young skull; D. Blainville stating that an old skull from New Guinea resembles that of the wild pigs of Malabar, and these belong to the *S. scrofa* type, which is different from the Chinese domestic breed. The latter has not been found in a wild condition.—Believe me, dear Wallace, yours very sincerely,

CH. DARWIN.

9 St. Mark's Crescent, Regent's Park, N.W. Sept. 18, 1865.

Dear Darwin,—I should have written before to thank you for the copy of your paper on climbing plants, which I read with great interest; I can imagine how much pleasure the working out must have given you. I was afraid you were too ill to make it advisable that you should be bothered with letters.

I write now, in hopes you are better, to communicate a curious case of *variation* becoming at once *hereditary*, which was brought forward at the British Association. I send a note of it on the other side, but if you would like more exact particulars, with names and dates and a drawing of the bird, I am sure Mr. O'Callaghan would send them to you.

I hope to hear that you are better, and that your new book is really to come out next winter.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

NOTE.—Last spring Mr. O'Callaghan was told by a country boy that he had seen a blackbird with a topknot; on which Mr. O'C. very judiciously told him to watch it and communicate further with him. After a time the boy told him he had found a

blackbird's nest, and had seen this crested bird near it and believed he belonged to it. He continued watching the nest till the young were hatched. After a time he told Mr. O'C. that two of the young birds seemed as if they would have topknots. He was told to get one of them as soon as it was fledged. However, he was too late, and they left the nest, but luckily he found them near and knocked one down with a stone, which Mr. O'C. had stuffed and exhibited. It has a fine crest, something like that of a Polish fowl, but *larger* in proportion to the bird, and very regular and well formed. The male must have been almost like the Umbrella bird in miniature, the crest is so large and expanded.

—A. R. W.

Down, Bromley, Kent, S.E. September 22, 1865.

Dear Wallace,—I am much obliged for your extract; I never heard of such a case, though such a variation is perhaps the most likely of any to occur in a state of nature and be inherited, inasmuch as all domesticated birds present races with a tuft or with reversed feathers on their heads. I have sometimes thought that the progenitor of the whole class must have been a crested animal.

Do you make any progress with your Journal of travels? I am the more anxious that you should do so as I have lately read with much interest some papers by you on the ouran-outang, etc., in the *Annals*, of which I have lately been reading the latter volumes. I have always thought that Journals of this nature do considerable good by advancing the taste for natural history; I know in my own case that nothing ever stimulated my zeal so much as reading Humboldt's Personal Narrative. I have not yet received the last part of *Linnean Transactions*, but your paper¹ at present will be rather beyond my strength, for though somewhat better I can as yet do hardly anything but lie on the sofa and be read aloud to. By the way, have you read Tylor and Lecky?² Both these books have interested me much. I suppose you have read Lubbock.³ In the last chapter

¹ Probably the one on the Distribution of Malayan Butterflies, *Linn. Soc. Trans.*, xxv.

² Tylor, "Early History of Mankind," and Lecky's "Rationalism."

³ "Prehistoric Times."

there is a note about you in which I most cordially concur.¹ I see you were at the British Association, but I have heard nothing of it except what I have picked up in the *Reader*. I have heard a rumour that the *Reader* is sold to the Anthropological Society. If you do not begrudge the trouble of another note (for my sole channel of news through Hooker is closed by his illness), I should much like to hear whether the *Reader* is thus sold. I should be very sorry for it, as the paper would thus become sectional in its tendency. If you write, tell me what you are doing yourself.

The only news which I have about the "Origin" is that Fritz Müller published a few months ago a remarkable book² in its favour, and secondly that a second French edition is just coming out.—Believe me, dear Wallace, yours very sincerely,

CH. DARWIN.

9 St. Mark's Crescent, Regent's Park. October 2, 1865.

Dear Darwin,—I was just leaving town for a few days when I received your letter, or should have replied at once.

The *Reader* has no doubt changed hands, and I am inclined to think for the better. It is purchased, I believe, by a gentleman who is a Fellow of the Anthropological Society, but I see no signs of its being made a special organ of that Society. The Editor (and, I believe, proprietor) is a Mr. Bendyshe, the most talented man in the Society, and, judging from his speaking, which I have often heard, I should say the articles on "Simeon and Simony," "Metropolitan Sewage," and "France and Mexico," are his, and these are in my opinion superior to anything that has been in the *Reader* for a long time; they have the point and brilliancy which are wanted to make leading articles readable and popular. The articles on Mill's Political Economy and on Mazzini are also first rate. He has introduced also the plan of having two, and now three, important articles in each number—one political or social, one literary, and one scientific. Under the old régime they never had an editor above mediocrity, except Masson (? Musson); there was a want of unity among the proprietors as to the aims and objects of the journal; and there was

¹ The note speaks of the "characteristic unselfishness" with which Wallace ascribed the theory of Natural Selection to Darwin.

² "Für Darwin."

a want of capital to secure the services of good writers. This seems to me to be now all changed for the better, and I only hope the rumour of that *bête noire*, the Anthropological Society, having anything to do with it may not cause our best men of science to withdraw their support and contributions.

I have read Tylor and am reading Lecky. I found the former somewhat disconnected and unsatisfactory from the absence of any definite result or any decided opinion on most of the matters treated of.

Lecky I like much, though he is rather tedious and obscure at times. Most of what he says has been said so much more forcibly by Buckle, whose work I have read for the second time with increased admiration, although with a clear view of some of his errors. Nevertheless, his is I think unapproachably the grandest work of the present century, and the one most likely to liberalise opinion. Lubbock's book is very good, but his concluding chapter very weak. Why are men of science so dreadfully afraid to say what they think and believe?

In reply to your kind inquiries about myself, I can only say that I am ashamed of my laziness. I have done nothing lately but write a paper on Pigeons for the *Ibis*, and am drawing up a Catalogue of my Collection of Birds.

As to my "Travels," I cannot bring myself to undertake them yet, and perhaps never shall, unless I should be fortunate enough to get a wife who would incite me thereto and assist me therein—which is not likely.

I am glad to hear that the "Origin" is still working its revolutionary way on the Continent. Will Müller's book on it be translated?

I am glad to hear you are a little better. My poor friend Spruce is still worse than you are, and I fear now will not recover. He wants to write a book if he gets well enough.—With best wishes, believe me, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. January 22, 1866.

My dear Wallace,—I thank you for your paper on Pigeons,¹ which interested me, as everything that you write does. Who

¹ "On the Pigeons of the Malay Archipelago," *The Ibis*. October, 1865. Mr. Wallace points out (p. 366) that "the most striking superabundance of

would ever have dreamed that monkeys influenced the distribution of pigeons and parrots! But I have had a still higher satisfaction; for I finished yesterday your paper in the *Linnean Transactions*.¹ It is admirably done. I cannot conceive that the most firm believer in Species could read it without being staggered. Such papers will make many more converts among naturalists than long-winded books such as I shall write if I have strength.

I have been particularly struck with your remarks on dimorphism; but I cannot quite understand one point (p. 22), and should be grateful for an explanation, for I want fully to understand you.² How can one female form be selected and the intermediate forms die out, without also the other extreme form also dying out from not having the advantages of the first selected form? for, as I understand, both female forms occur on the same island. I quite agree with your distinction be-

pigeons as well as of parrots, is confined to the Australo-Malayan sub-regions in which . . . the forest-haunting and fruit-eating mammals, such as monkeys and squirrels, are totally absent." He points out also that monkeys are "exceedingly destructive to eggs and young birds."—Note, "More Letters," i. 265.

¹ "The Geographical Distribution and Variability of the Malayan Papiionidæ," *Linn. Soc. Trans.*, xxv.

² The passage referred to in this letter as needing further explanation is the following: "The last six cases of mimicry are especially instructive, because they seem to indicate one of the processes by which dimorphic forms have been produced. When, as in these cases, one sex differs much from the other, and varies greatly itself, it may be that individual variations will occasionally occur, having a distant resemblance to groups which are the objects of mimicry, and which it is therefore advantageous to resemble. Such a variety will have a better chance of preservation: the individuals possessing it will be multiplied; and their accidental likeness to the favoured group will be rendered permanent by hereditary transmission, and each successive variation which increases the resemblance being preserved, and all variation departing from the favoured type having less chance of preservation, there will in time result those singular cases of two or more isolated and fixed forms bound together by that intimate relationship which constitutes them the sexes of a single species. The reason why the females are more subject to this kind of modification than the males is probably, that their slower flight, when laden with eggs, and their exposure to attack while in the act of depositing their eggs upon leaves render it especially advantageous for them to have additional protection. This they at once obtain by acquiring a resemblance to other species which, from whatever cause, enjoy a comparative immunity from persecution."

tween dimorphic forms and varieties; but I doubt whether your criterion of dimorphic forms not producing intermediate offspring will suffice; for I know of a good many varieties, which must be so called, that will not blend or intermix, but produce offspring quite like either parent.

I have been particularly struck with your remarks on geological distribution in Celebes. It is impossible that anything could be better put and it would give a cold shudder to the immutable naturalists.

And now I am going to ask a question which you will not like. How does your Journal get on? It will be a shame if you do not popularise your researches.

My health is so far improved that I am able to work one or two hours a day.—Believe me, dear Wallace, yours very sincerely,
CH. DARWIN.

9 St. Mark's Crescent, Regent's Park, N.W. February 4, 1866.

My dear Darwin,—I am very glad to hear you are a little better, and hope we shall soon have the pleasure of seeing your volume on "Variation under Domestication." I do not see the difficulty you seem to feel about two or more female forms of one species. The *most common* or *typical* female form must have certain characters or qualities which are sufficiently advantageous to it to enable it to maintain its existence; in general, such as vary much from it die out. But occasionally a variation may occur which has special advantageous characters of its own (such as mimicking a protected species), and then this variation will maintain itself by selection. In no less than three of my *polymorphic* species of *Papilio*, one of the female forms mimics the *Polydorous* group, which, like the *Æneas* group in America, seem to have some special protection. In two or three other cases one of the female forms is confined to a restricted locality, to the conditions of which it is probably specially adapted. In other cases one of the female forms resembles the male, and perhaps receives a protection from the abundance of the males, in the crowd of which it is passed over. I think these considerations render the production of two or three forms of female very conceivable. The physiological difficulty is to me greater, of how each of two forms of female produces offspring like the other female as well as like itself, but no intermediates?

If you "know varieties that will not blend or intermix, but produce offspring quite like either parents," is not that the very physiological test of a species which is wanting for the *complete proof* of the origin of species?

I have by no means given up the idea of writing my *Travels*, but I think I shall be able to do it better for the delay, as I can introduce chapters giving popular sketches of the subjects treated of in my various papers.

I hope, if things go as I wish this summer, to begin work at it next winter. But I feel myself incorrigibly lazy, and have no such system of collecting and arranging facts or of making the most of my materials as you and many of our hard-working naturalists possess in perfection.—With best wishes, believe me, dear Darwin, yours most sincerely,

ALFRED R. WALLACE.

Down, Bromley, S.E. Tuesday, February, 1866.

My dear Wallace,—After I had dispatched my last note, the simple explanation which you give had occurred to me, and seems satisfactory. I do not think you understand what I mean by the non-blending of certain varieties. It does not refer to fertility. An instance will explain. I crossed the Painted Lady and Purple sweet peas, which are very differently coloured varieties, and got, even out of the same pod, both varieties perfect, but none intermediate. Something of this kind, I should think, must occur at first with your butterflies and the three forms of lythrum; though these cases are in appearance so wonderful, I do not know that they are really more so than every female in the world producing distinct male and female offspring.

I am heartily glad that you mean to go on preparing your *Journal*.—Believe me, yours very sincerely,

CH. DARWIN.

Hurstpierpoint, Sussex. July 2, 1866.

My dear Darwin,—I have been so repeatedly struck by the utter inability of numbers of intelligent persons to see clearly, or at all, the self-acting and necessary effects of Natural Selection, that I am led to conclude that the term itself, and your mode of illustrating it, however clear and beautiful to many of

us, are yet not the best adapted to impress it on the general naturalist public. The two last cases of this misunderstanding are (1) the article on "Darwin and His Teachings" in the last *Quarterly Journal of Science*, which, though very well written and on the whole appreciative, yet concludes with a charge of something like blindness, in your not seeing that Natural Selection requires the constant watching of an intelligent "chooser" like man's selection to which you so often compare it; and (2) in Janet's recent work on the "Materialism of the Present Day," reviewed in last Saturday's *Reader*, by an extract from which I see that he considers your weak point to be that you do not see that "thought and direction are essential to the action of Natural Selection." The same objection has been made a score of times by your chief opponents, and I have heard it as often stated myself in conversation. Now, I think this arises almost entirely from your choice of the term Natural Selection, and so constantly comparing it in its effects to man's selection, and also to your so frequently personifying nature as "selecting," as "preferring," as "seeking only the good of the species," etc., etc. To the few this is as clear as daylight, and beautifully suggestive, but to many it is evidently a stumbling-block. I wish, therefore, to suggest to you the possibility of entirely avoiding this source of misconception in your great work (if not now too late), and also in any future editions of the "Origin," and I think it may be done without difficulty and very effectually by adopting Spencer's term (which he generally uses in preference to Natural Selection), viz. "Survival of the Fittest." This term is the plain expression of the *fact*; Natural Selection is a metaphorical expression of it, and to a certain degree *indirect* and *incorrect*, since, even personifying Nature, she does not so much select special variations as exterminate the most unfavourable ones.

Combined with the enormous multiplying powers of all organisms, and the "struggle for existence," leading to the constant destruction of by far the largest proportion—facts which no one of your opponents, as far as I am aware, has denied or misunderstood—"the survival of the fittest," rather than of those which were less fit, could not possibly be denied or misunderstood. Neither would it be possible to say that to ensure the "survival of the fittest" any *intelligent chooser* was necessary, whereas when you say Natural Selection acts so as to choose

those that are fittest it *is* misunderstood, and apparently always will be. Referring to your book, I find such expressions as "Man selects only for his own good; Nature only for that of the being which she tends." This, it seems, will always be misunderstood; but if you had said, "Man selects only for his own good; Nature by the inevitable survival of the fittest, only for that of the being she tends," it would have been less liable to be so.

I find you use the term Natural Selection in two senses—(1) for the simple preservation of favourable and rejection of unfavourable variations, in which case it is equivalent to "survival of the fittest"; (2) for the *effect or change* produced by this preservation, as when you say, "To sum up the circumstances favourable or unfavourable to natural selection," and, again, "Isolation, also, is an important element in the process of natural selection": here it is not merely "survival of the fittest," but *change* produced by survival of the fittest, that is meant. On looking over your fourth chapter, I find that these alterations of terms can be in most cases easily made, while in some cases the addition of "or survival of the fittest" after "natural selection" would be best; and in others, less likely to be misunderstood, the original term might stand alone.

I could not venture to propose to any other person so great an alteration of terms, but you, I am sure, will give it an impartial consideration, and, if you really think the change will produce a better understanding of your work, will not hesitate to adopt it. It is evidently also necessary not to personify "nature" too much, though I am very apt to do it myself, since people will not understand that all such phrases are metaphors. Natural Selection is, when understood, so necessary and self-evident a principle that it is a pity it should be in any way obscured; and it therefore occurs to me that the free use of "survival of the fittest," which is a compact and accurate definition of it, would tend much to its being more widely accepted and prevent its being so much misrepresented and misunderstood.

There is another objection made by Janet which is also a very common one. It is that the chances are almost infinite against the particular kind of variation required being coincident with each change of external conditions, to enable an animal to become modified by Natural Selection in harmony with such changed conditions; especially when we consider that, to have

produced the almost infinite modifications of organic beings, this coincidence must have taken place an almost infinite number of times.

Now it seems to me that you have yourself led to this objection being made by so often stating the case too strongly against yourself. For example, at the commencement of Chapter IV. you ask if it is "improbable that useful variations should sometimes occur in the course of thousands of generations"; and a little further on you say, "unless profitable variations do occur, natural selection can do nothing." Now, such expressions have given your opponents the advantage of assuming that *favourable* variations are *rare accidents*, or may even for long periods never occur at all, and thus Janet's argument would appear to many to have great force. I think it would be better to do away with all such qualifying expressions, and constantly maintain (what I certainly believe to be the fact) that *variations of every kind are always occurring in every part of every species*, and therefore that favourable variations are *always ready* when wanted. You have, I am sure, abundant materials to prove this, and it is, I believe, the grand fact that renders modification and adaptation to conditions almost always possible. I would put the burthen of proof on my opponents to show that any one organ, structure, or faculty, does *not vary*, even during one generation, among all the individuals of a species; and also to show any *mode or way* in which any such organ, etc., does not vary. I would ask them to give any reason for supposing that any organ, etc., is ever *absolutely identical* at any *one time in all the individuals* of a species, and if not, then it is always varying, and there are always materials which, from the simple fact that the "fittest survive," will tend to the modification of the race into harmony with changed conditions.

I hope these remarks may be intelligible to you, and that you will be so kind as to let me know what you think of them.

I have not heard for some time how you are getting on. I hope you are still improving in health, and that you will be able now to get on with your great work, for which so many thousands are looking with interest.—With best wishes, believe me, my dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. July 5, [1866].

My dear Wallace,—I have been much interested by your letter, which is as clear as daylight. I fully agree with all that you say on the advantages of H. Spencer's excellent expression of "the survival of the fittest." This, however, had not occurred to me till reading your letter. It is, however, a great objection to this term that it cannot be used as a substantive governing a verb; and that this is a real objection I infer from H. Spencer continually using the words "Natural Selection."

I formerly thought, probably in an exaggerated degree, that it was a great advantage to bring into connection natural and artificial selection; this indeed led me to use a term in common, and I still think it some advantage. I wish I had received your letter two months ago, for I would have worked in "the survival," etc., often in the new edition of the "Origin," which is now almost printed off, and of which I will, of course, send you a copy. I will use the term in my next book on Domestic Animals, etc., from which, by the way, I plainly see that you expect *much* too much. The term Natural Selection has now been so largely used abroad and at home that I doubt whether it could be given up, and with all its faults I should be sorry to see the attempt made. Whether it will be rejected must now depend "on the survival of the fittest."

As in time the term must grow intelligible, the objections to its use will grow weaker and weaker. I doubt whether the use of any term would have made the subject intelligible to some minds, clear as it is to others; for do we not see, even to the present day, Malthus on Population absurdly misunderstood? This reflection about Malthus has often comforted me when I have been vexed at the misstatement of my views.

As for M. Janet,¹ he is a metaphysician, and such gentlemen are so acute that I think they often misunderstand common folk. Your criticism on the double sense in which I have used Natural Selection is new to me and unanswerable; but my blunder has done no harm, for I do not believe that anyone excepting you has ever observed it. Again, I agree that I have said too much about "favourable variations," but I am inclined to think you put the opposite side too strongly; if every part

¹ This no doubt refers to Janet's "*Matérialisme Contemporain*."

of every being varied, I do not think we should see the same end or object gained by such wonderfully diversified means.

I hope you are enjoying the country and are in good health, and are working hard at your Malay Archipelago book, for I will always put this wish in every note I write to you, like some good people always put in a text. My health keeps much the same, or rather improves, and I am able to work some hours daily.—With many thanks for your interesting letter, believe me, my dear Wallace, yours sincerely,

CH. DARWIN.

P.S.—I suppose you have read the last number of H. Spencer; I have been struck with astonishment at the prodigality of original thought in it. But how unfortunate it is that it seems scarcely ever possible to discriminate between the direct effect of external influences and the “survival of the fittest.”

9 St. Mark's Crescent, Regent's Park, N.W. Nov. 19, 1866.

Dear Darwin,—Many thanks for the fourth edition of the “Origin,” which I am glad to see grows so vigorously at each moult, although it undergoes no metamorphosis. How curious it is that Dr. Wells should so clearly have seen the principle of Natural Selection fifty years ago, and that it should have struck no one that it was a great principle of universal application in Nature!

We are going to have a discussion on “Mimicry, as producing Abnormal Sexual Characters,” at the Entomological to-night. I have a butterfly (*Diadema*) of which the female is metallic blue, the male dusky brown, contrary to the rule in all other species of the genus, and in almost all insects; but the explanation is easy—it mimics a metallic *Euplœa*, and so gets a protection perhaps more efficient than its allies derive from their sombre colours, and which females require much more than males. I read a paper on this at the British Association. Have you the report published at Nottingham in a volume by Dr. Robertson? If so, you can tell me if my paper is printed in full.

I suppose you have read Agassiz's marvellous theory of the Great Amazonian glacier, 2,000 miles long! I presume that will be a *little* too much, even for you. I have been writing a little popular paper on “Glacial Theories” for the *Quarterly Journal*

of *Science* of January next, in which I stick up for glaciers in North America and icebergs in the Amazon!

I was very glad to hear from Lubbock that your health is permanently improved. I hope therefore you will be able to give us a volume per annum of your *magnum opus*, with all the facts as you now have them, leaving additions to come in new editions.

I am working a little at another family of my butterflies, and find the usual interesting and puzzling cases of variation, but no such phenomena as in the Papilionidæ—With best wishes, believe me, my dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

6 Queen Anne Street, W. Monday, January, 1867.

My dear Wallace,—I return by this post the *Journal*.¹ Your résumé of glacier action seems to me very good, and has interested my brother much, and as the subject is new to him he is a better judge. That is quite a new and perplexing point which you specify about the freshwater fishes during the glacial period.

I have also been very glad to see the article on Lyell, which seems to me to be done by some good man.

I forgot to say when with you, but I then indeed did not know so much as I do now, that the sexual, i.e. *ornamental*, differences in fishes, which differences are sometimes very great, offer a difficulty in the wide extension of the view that the female is not brightly coloured on account of the danger which she would incur in the propagation of the species.

I very much enjoyed my long conversation with you; and to-day we return home, and I to my horrid dull work of correcting proof-sheets.—Believe me, my dear Wallace, yours very sincerely,

CHARLES DARWIN.

P.S.—I had arranged to go and see your collection on Saturday evening, but my head suddenly failed after luncheon, and I was forced to lie down all the rest of the day.

Down, Bromley, Kent, S.E. February 23, 1867.

Dear Wallace,—I much regretted that I was unable to call on you, but after Monday I was unable even to leave the house.

¹ *Quarterly Journal of Science*, January 7, 1867. "Ice marks in North Wales," by A. R. Wallace.

On Monday evening I called on Bates and put a difficulty before him, which he could not answer, and, as on some former similar occasion, his first suggestion was, "You had better ask Wallace." My difficulty is, why are caterpillars sometimes so beautifully and artistically coloured? Seeing that many are coloured to escape danger, I can hardly attribute their bright colour in other cases to mere physical conditions. Bates says the most gaudy caterpillar he ever saw in Amazonia (of a Sphinx) was conspicuous at the distance of yards from its black and red colouring whilst feeding on large green leaves. If anyone objected to male butterflies having been made beautiful by sexual selection, and asked why should they not have been made beautiful as well as their caterpillars, what would you answer? I could not answer, but should maintain my ground. Will you think over this, and some time, either by letter or when we meet, tell me what you think? Also, I want to know whether your *female* mimetic butterfly is more beautiful and brighter than the male?

When next in London I must get you to show me your Kingfishers.

My health is a dreadful evil; I failed in half my engagements during this last visit to London.—Believe me, yours very sincerely,
C. DARWIN.

The answer to this letter is missing, but in Vol. II. of "My Life," p. 3, Wallace writes:

"On reading this letter I almost at once saw what seemed to be a very easy and probable explanation of the facts. I had then just been preparing for publication (in the *Westminster Review*) my rather elaborate paper on "Mimicry and Protective Colouring," and the numerous cases in which specially showy and slow-flying butterflies were known to have a peculiar odour and taste which protected them from the attacks of insect-eating birds and other animals led me at once to suppose that the gaudily coloured caterpillars must have a similar protection. I had just ascertained from Mr. Jenner Weir that one of our common white moths (*Spilosoma menthastri*) would not be eaten by most of the small birds in his aviary, nor by young turkeys. Now, as a *white* moth is as conspicuous in the dusk

as a coloured caterpillar in the daylight, this case seemed to me so much on a par with the other that I felt almost sure my explanation would turn out correct. I at once wrote to Mr. Darwin to this effect."

Down, Bromley, Kent, S.E. February 26, 1867.

My dear Wallace,—Bates was quite right, you are the man to apply to in a difficulty. I never heard anything more ingenious than your suggestion, and I hope you may be able to prove it true. That is a splendid fact about the white moths; it warms one's very blood to see a theory thus almost proved to be true.¹ With respect to the beauty of male butterflies, I must as yet think that it is due to sexual selection; there is some evidence that dragonflies are attracted by bright colours; but what leads me to the above belief is so many male Orthoptera and Cicadas having musical instruments. This being the case, the analogy of birds makes me believe in sexual selection with respect to colour in insects. I wish I had strength and time to make some of the experiments suggested by you; but I thought butterflies would not pair in confinement; I am sure I have heard of some such difficulty. Many years ago I had a dragonfly painted with gorgeous colours, but I never had an opportunity of fairly trying it.

The reason of my being so much interested just at present about sexual selection is that I have almost resolved to publish a little essay on the Origin of Mankind, and I still strongly think (though I failed to convince you, and this to me is the heaviest blow possible) that sexual selection has been the main agent in forming the races of man.

By the way, there is another subject which I shall introduce in my essay, viz. expression of countenance. Now, do you happen to know by any odd chance a very good-natured and acute observer in the Malay Archipelago who, you think, would make a few easy observations for me on the expression of the Malays when excited by various emotions. For in this case I would send to such person a list of queries. I thank you for your most interesting letters and remain, yours very sincerely,

CH. DARWIN.

¹ The suggestion that conspicuous caterpillars or perfect insects (e.g. white butterflies) which are distasteful to birds, are protected by being easily recognised and avoided.

9 *St. Mark's Crescent, N.W. March 11, 1867.*

Dear Darwin,—I return your queries, but cannot answer them with any certainty. For the Malays I should say Yes to 1, 3, 8, 9, 10 and 17, and No to 12, 13 and 16; but I cannot be *certain* in any one. But do you think these things are of much importance? I am inclined to think that if you could get good direct observations you would find some of them often differ from tribe to tribe, from island to island, and sometimes from village to village. Some no doubt may be deep-seated, and would imply organic differences; but can you tell beforehand which these are? I presume the Frenchman shrugs his shoulders whether he is of the Norman, Breton, or Gaulish stock. Would it not be a good thing to send your List of Queries to some of the Bombay and Calcutta papers? as there must be numbers of Indian judges and other officers who would be interested and would send you hosts of replies. The Australian papers and New Zealand might also publish them, and then you would have a fine basis to go on.

Is your essay on Variation in Man to be a supplement to your volume on Domesticated Animals and Cultivated Plants? I would rather see your second volume on "The Struggle for Existence, etc.," for I doubt if we have a sufficiency of fair and accurate facts to do anything with man. Huxley, I believe, is at work upon it.

I have been reading Murray's volume on the Geographical Distribution of Mammals. He has some good ideas here and there, but is quite unable to understand Natural Selection, and makes a most absurd mess of his criticism of your views on oceanic islands.

By the bye, what an interesting volume the whole of your materials on that subject would, I am sure, make.—Yours very sincerely,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. March, 1867.

My dear Wallace,—I thank you much for your two notes. The case of Julia Pastrana¹ is a splendid addition to my other cases of correlated teeth and hair, and I will add it in correcting

¹ A bearded woman having an irregular double set of teeth. See "Animals and Plants," ii. 328.

the proof of my present volume. Pray let me hear in course of the summer if you get any evidence about the gaudy caterpillars. I should much like to give (or quote if published) this idea of yours, if in any way supported, as suggested by you. It will, however, be a long time hence, for I can see that sexual selection is growing into quite a large subject, which I shall introduce into my essay on man, supposing that I ever publish it.

I had intended giving a chapter on Man, inasmuch as many call him (not *quite* truly) an eminently *domesticated* animal; but I found the subject too large for a chapter. Nor shall I be capable of treating the subject well, and my sole reason for taking it up is that I am pretty well convinced that sexual selection has played an important part in the formation of races, and sexual selection has always been a subject which has interested me much.

I have been very glad to see your impression from memory on the expressions of Malays. I fully agree with you that the subject is in no way an important one: it is simply a "hobby-horse" with me about twenty-seven years old; and after thinking that I would write an essay on Man, it flashed on me that I could work in some "supplemental remarks on expression." After the horrid, tedious, dull work of my present huge and, I fear, unreadable book, I thought I would amuse myself with my hobby-horse. The subject is, I think, more curious and more amenable to scientific treatment than you seem willing to allow. I want, anyhow, to upset Sir C. Bell's view, given in his most interesting work, "The Anatomy of Expression," that certain muscles have been given to man solely that he may reveal to other men his feelings. I want to try and show how expressions have arisen.

That is a good suggestion about newspapers; but my experience tells me that private applications are generally most fruitful. I will, however, see if I can get the queries inserted in some Indian paper. I do not know names or addresses of any other papers.

I have just ordered, but not yet received, Murray's book: Lindley used to call him a blunder-headed man. It is very doubtful whether I shall ever have strength to publish the latter part of my materials.

My two female amanuenses are busy with friends, and I fear this scrawl will give you much trouble to read.—With many thanks, yours very sincerely,

CH. DARWIN.

Down, Bromley, Kent, S.E. April 29, 1867.

Dear Wallace,—I have been greatly interested by your letter;¹ but your view is not new to me. If you will look at p. 240 of the fourth edition of the "Origin," you will find it very briefly given with two extremes of the peacock and black grouse. A more general statement is given at p. 101, or at p. 89 of the first edition, for I have long entertained this view, though I have never had space to develop it. But I had not sufficient knowledge to generalise as far as you do about colouring and nesting. In your paper, perhaps you will just allude to my scanty remark in the fourth edition, because in my Essay upon Man I intend to discuss the whole subject of sexual selection, explaining, as I believe it does, much with respect to man. I have collected all my old notes and partly written my discussion, and it would be flat work for me to give the leading idea as exclusively from you. But as I am sure from your greater knowledge of ornithology and entomology that you will write a much better discussion than I could, your paper will be of great use to me. Nevertheless, I must discuss the subject fully in my Essay on Man. When we met at the Zoological Society and I asked you about the sexual differences in kingfishers, I had this subject in view; as I had when I suggested to Bates the difficulty about gaudy caterpillars which you have so admirably (as I believe it will prove) explained. I have got one capital case (genus forgotten) of an Australian bird in which the female has long-tailed plumes and which consequently builds a different nest from all her allies.² With respect to certain female birds being more brightly coloured than the males, and the latter incubating, I have gone

¹ The letter to which this is a reply is missing. It evidently refers to Wallace's belief in the paramount importance of protection in the evolution of colour. See also Darwin's letter of February 26, 1867.

² *Menura superba*. See "The Descent of Man" (1901), p. 687. *Rhynchæa*, mentioned on p. 184, is discussed in the "Descent," p. 727. The female is more brightly coloured than the male and has a convoluted trachea, elsewhere a masculine character. There seems some reason to suppose that "the male undertakes the duty of incubation."

a little into the subject and cannot say that I am fully satisfied. I remember mentioning to you the case of *Rhynchæa*, but its nesting seems unknown. In some other cases the difference in brightness seemed to me hardly sufficiently accounted for by the principle of protection. At the Falkland Islands there is a carrion hawk in which the female (as I ascertained by dissection) is the brightest coloured, and I doubt whether protection will here apply; but I wrote several months ago to the Falklands to make inquiries. The conclusions to which I have been leaning is that in some of these abnormal cases the colour happened to vary in the female alone, and was transmitted to females alone, and that her variations have been selected through the admiration of the male.

It is a very interesting subject, but I shall not be able to go on with it for the next five or six months, as I am fully employed in correcting dull proof-sheets; when I return to the work I shall find it much better done by you than I could have succeeded in doing.

With many thanks for your very interesting note, believe me,
 dear Wallace, yours very sincerely, CH. DARWIN.

It is curious how we hit on the same ideas. I have endeavoured to show in my MS. discussion that nearly the same principles account for young birds *not* being gaily coloured in many cases, but this it too complex a point for a note.

Postscript.

Down, April 29.

My dear Wallace,—On reading over your letter again, and on further reflection, I do not think (as far as I remember my words) that I expressed myself *nearly strongly* enough as to the value and beauty of your generalisation, viz. that all birds in which the female is conspicuously or brightly coloured build in holes or under domes. I thought that this was the explanation in many, perhaps most cases, but do not think I should ever have extended my view to your generalisation. Forgive me troubling you with this P.S.—Yours,

CH. DARWIN.

Down, Bromley, Kent, S.E. May 5, 1867.

My dear Wallace,—The offer of your valuable notes is *most* generous, but it would vex me to take so much from you, as it is

certain that you could work up the subject very much better than I could. Therefore I earnestly and without any reservation hope that you will proceed with your paper, so that I return your notes.

You seem already to have well investigated the subject. I confess on receiving your note that I felt rather flat at my recent work being almost thrown away, but I did not intend to show this feeling. As a proof how little advance I had made on the subject, I may mention that though I had been collecting facts on the colouring and other sexual differences in Mammals, your explanation with respect to the females had not occurred to me. I am surprised at my own stupidity, but I have long recognised how much clearer and deeper your insight into matters is than mine.

I do not know how far you have attended to the laws of inheritance, so what follows may be obvious to you. I have begun my discussion on sexual selection by showing that new characters often appear in one sex and are transmitted to that sex alone, and that from some unknown cause such characters apparently appear oftener in the male than in the female. Secondly characters may be developed and be confined to the male, and long afterwards be transferred to the female. Thirdly, characters may again arise in either sex and be transmitted to both sexes, either in an equal or unequal degree. In this latter case I have supposed that the survival of the fittest has come into play with female birds and kept the female dull-coloured. With respect to the absence of spurs in female gallinaceous birds, I presume that they would be in the way during incubation; at least, I have got the case of a German breed of fowls in which the hens were spurred, and were found to disturb and break their eggs much.

With respect to the females of deer not having horns, I presume it is to save the loss of organised matter.

In your note you speak of sexual selection and protection as sufficient to account for the colouring of all animals; but it seems to me doubtful how far this will come into play with some of the lower animals, such as sea anemones, some corals, etc., etc.

On the other hand, Haeckel has recently well shown that the transparency and absence of colour in the lower oceanic animals, belonging to the most different classes, may be well accounted for on the principle of protection.

Some time or other I should like much to know where your paper on the nests of birds has appeared, and I shall be extremely anxious to read your paper in the *Westminster Review*.

Your paper on the sexual colouring of birds will, I have no doubt, be very striking.

Forgive me, if you can, for a touch of illiberality about your paper, and believe me, yours very sincerely,

CH. DARWIN.

Down, Bromley, Kent, S.E. July 6, 1867.

My dear Wallace,—I am very much obliged for your article on Mimicry,¹ the whole of which I have read with the greatest interest. You certainly have the art of putting your ideas with remarkable force and clearness; now that I am slaving over proof-sheets it makes me almost envious.

I have been particularly glad to read about the birds' nests, and I must procure the *Intellectual Observer*; but the point which I think struck me most was about its being of no use to the Heliconias to acquire in a slight degree a disagreeable taste. What a curious case is that about the coral snakes. The summary, and indeed the whole, is excellent, and I have enjoyed it much.—With many thanks, yours very sincerely,

CH. DARWIN.

9 St. Mark's Crescent, N.W.

Wednesday (August or September, 1867.)

Dear Darwin,—I am very sorry I was out when you called yesterday. I had just gone to the Zoological Gardens, and I met Sir C. Lyell, who told me you were in town.

If you should have time to go to Bayswater, I think you would be pleased to see the collections which I have displayed there in the form of an *exhibition* (though the public will not go to see it).

If you can go with any friends, I should like to meet you there if you can appoint a time.

I am glad to find you continue in tolerable health.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

¹ *Westminster Review*, July, 1867.

What do you think of the Duke of Argyll's criticisms, and the more pretentious one in the last number of the *North British Review*?

I have written a little article answering them both, but I do not yet know where to get it published.—A. R. W.

76½ Westbourne Grove, Bayswater, W. October 1, 1867.

Dear Darwin,—I am sorry I was not in town when your note came. I took a short trip to Scotland after the British Association Meeting, and went up Ben Lawers. It was very cold and wet, and I could not find a companion, or I should have gone as far as Glen Roy.

My article on "Creation by Law," in reply to the Duke of Argyll and the *North British* reviewer, is in the present month's number of the *Quarterly Journal of Science*. I cannot send you a copy because they do not allow separate copies to be printed.

There is a nice illustration of the *predicted* Madagascar moth and *Angræcum sesquipedale*.

I shall be glad to know whether I have done it satisfactorily to you, and hope you will not be so very sparing of criticism as you usually are.

I hope you are getting on well with your great book. I hear a rumour that we are to have *one* vol. of it about Christmas.

I quite forget whether I told you that I have a little boy, now three months old, and have named him Herbert Spencer (having had a brother Herbert). I am now staying chiefly in the country at Hurstpierpoint, but come up to town once a month at least. You may address simply, "Hurstpierpoint, Sussex."

Hoping your health is tolerable and that all your family are well, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. October 12 and 13, 1867.

My dear Wallace,—I ordered the journal a long time ago, but by some oversight received it only yesterday and read it. You will think my praise not worth having from being so indiscriminate, but if I am to speak the truth, I must say I admire every word.

You have just touched on the points which I particularly wished to see noticed. I am glad you had the courage to take up *Angræcum*¹ after the Duke's attack; for I believe the principle in this case may be widely applied. I like the figure, but I wish the artist had drawn a better sphinx.

With respect to beauty, your remarks on hideous objects and on flowers not being made beautiful except when of practical use to them strike me as very good.

On this one point of beauty, I can hardly think that the Duke was quite candid. I have used in the concluding paragraph of my present book precisely the same argument as you have, even bringing in the bulldog,² with respect to variations not having been specially ordained. Your metaphor of the river³ is new to me, and admirable; but your other metaphor, in which you compare classification and complex machines, does not seem to me quite appropriate, though I cannot point out what seems deficient. The point which seems to me strong is that all naturalists admit that there is a *natural* classification, and it is this which descent explains. I wish you had insisted a little

¹ *Angræcum sesquipedale*, a Madagascar orchid, with a whip-like nectary, 11 to 12 in. in length, which, according to Darwin ("Fertilisation of Orchids," 2nd Edit., p. 163), is adapted to the visits of a moth with a proboscis of corresponding length. He points out that there is no difficulty in believing in the existence of such a moth as F. Müller had described (*Nature*, 1873, p. 223), a Brazilian sphinx-moth with a trunk 10 to 11 in. in length. Moreover, Forbes has given evidence to show that such an insect does exist in Madagascar (*Nature*, 1873, p. 121). The case of *Angræcum* was put forward by the Duke of Argyll as being necessarily due to the personal contrivance of the Deity. Mr. Wallace shows (p. 476, *Quarterly Journal of Science*, 1867) that both proboscis and nectary might be increased in length by means of Natural Selection. It may be added that Herman Müller has shown good grounds for believing that mutual specialisation of this kind is beneficial both to insect and to plant.

² "Variation of Animals and Plants," 1st Edit., ii. 431. "Did he cause the frame and mental qualities of the dog to vary in order that a breed might be formed of indomitable ferocity, with jaws fitted to pin down the bull for man's brutal sport?"

³ See Wallace, *Quarterly Journ. of Sci.*, 1867, pp. 477-8. He imagined an observer examining a great river system, and finding everywhere adaptations which reveal the design of the Creator. "He would see special adaptations to the wants of man in the broad, quiet, navigable rivers, through fertile alluvial plains, that would support a large population, while the rocky streams and mountain torrents were confined to those sterile regions suitable for a small population of shepherds and herdsmen."

more against the *North British*¹ reviewer assuming that each variation which appears is a strongly marked one; though by implication you have made this *very* plain. Nothing in your whole article has struck me more than your view with respect to the limit of fleetness in the race-horse and other such cases; I shall try and quote you on this head in the proof of my concluding chapter. I quite missed this explanation, though in the case of wheat I hit upon something analogous. I am glad you praise the Duke's book, for I was much struck with it. The part about flight seemed to me at first very good, but as the wing is articulated by a ball-and-socket joint, I suspect the Duke would find it very difficult to give any reason against the belief that the wing strikes the air more or less obliquely. I have been very glad to see your article and the drawing of the butterfly in *Science Gossip*. By the way, I cannot but think that you push protection too far in some cases, as with the stripes on the tiger. I have also this morning read an excellent abstract in the *Gardener's Chronicle* of your paper on nests;² I was not by any means fully converted by your letter, but I think now I am so; and I hope it will be published somewhere *in extenso*. It strikes me as a capital generalisation, and appears to me even more original than it did at first.

I have had an excellent and cautious letter from Mr. Geach of Singapore with some valuable answers on expression, which I owe to you.

I heartily congratulate you on the birth of "Herbert Spencer," and may he deserve his name, but I hope he will copy his father's style and not his namesake's. Pray observe, though I fear I am a month too late, when tears are first secreted enough to overflow; and write down date.

I have finished Vol. I. of my book, and I hope the whole will

¹ At p. 485 Wallace deals with Fleeming Jenkin's review in the *North British Review*, 1867. The review strives to show that there are strict limitations to variation, since the most rigorous and long-continued selection does not indefinitely increase such a quality as the fleetness of a race-horse. On this Wallace remarks that the argument "fails to meet the real question," which is not whether indefinite change is possible, "but whether such differences as do occur in nature could have been produced by the accumulation of variations by selection.

² Abstract of a paper on "Birds' Nests and Plumage," read before the British Association. See *Gard. Chron.*, 1867, p. 1047.

be out by the end of November; if you have the patience to read it through, which is very doubtful, you will find, I think, a large accumulation of facts which will be of service to you in your future papers, and they could not be put to better use, for you certainly are a master in the noble art of reasoning.

Have you changed your house to Westbourne Grove?

Believe me, my dear Wallace, yours very sincerely,

CH. DARWIN.

This letter is so badly expressed that it is barely intelligible, but I am tired with proofs.

P.S.—Mr. Warington has lately read an excellent and spirited abstract of the "Origin" before the Victoria Institute, and as this is a most orthodox body he has gained the name of the devil's advocate. The discussion which followed during three consecutive meetings is very rich from the nonsense talked. If you would care to see the number I could lend it you.

I forgot to remark how capitally you turn the table on the Duke, when you make him create the *Angræcum* and moth by special creation.

Hurstpierpoint. October 22, 1867.

Dear Darwin,—I am very glad you approve of my article on "Creation by Law" as a whole.

The "machine metaphor" is not mine, but the *North British* reviewer's. I merely accept it and show that it is on our side and not against us, but I do not think it at all a good metaphor to be used as an *argument* either way. I did not half develop the argument on the limits of variation, being myself limited in space; but I feel satisfied that it is the true answer to the very common and very strong objection, that "variation has strict limits." The fallacy is the requiring variation in domesticity to go beyond the limits of the same variation under nature. It does do so sometimes, however, because the conditions of existence are so different. I do not think a case can be pointed out in which the limits of variation under domestication are not up to or beyond those already marked out in nature, only we generally get in the *species* an amount of change which in nature occurs only in the whole range of the *genus* or *family*.

The many cases, however, in which variation has gone far

beyond nature and has not yet stopped are ignored. For instance, no wild pomaceous fruit is, I believe, so large as our apples, and no doubt they could be got much larger if flavour, etc., were entirely neglected.

I may perhaps push "protection" too far sometimes, for it is my hobby just now, but as the lion and the tiger are, I think, the only two non-arboreal cats, I think the tiger stripe agreeing so well with its usual habitat is at least a probable case.

I am rewriting my article on Birds' Nests for the new *Natural History Review*.

I cannot tell you about the first appearance of *tears*, but it is very early—the first week or two, I think. I can see the *Victoria Institute Magazine* at the London Library.

I shall read your book, *every* word. I hear from Sir C. Lyell that you come out with a grand new theory at the end, which even the *cautious* (!) Huxley is afraid of! Sir C. said he could think of nothing else since he read it. I long to see it.

My address is Hurstpierpoint during the winter, and, when in town, 76½ Westbourne Grove.

I suppose you will now be going on with your book on Sexual Selection and Man, by way of relaxation! It is a glorious subject, but will require delicate handling.—Yours very faithfully,

ALFRED R. WALLACE.

10 Duchess Street, W. February 7, 1868.

Dear Darwin,—I have to thank you for signing the Memorial as to the East London Museum, and also for your kindness in sending me a copy of your great book, which I have only just received. I shall take it down in the country with me next week, and enjoy every line at my leisure.

Allow me also to congratulate you on the splendid position obtained by your second son at Cambridge.

You will perhaps be glad to hear that I have been for some time hammering away at my *Travels*, but I fear I shall make a mess of it. I shall leave most of the *Natural History* generalisation, etc., for another work, as if I wait to incorporate all, I may wait for years.—Hoping you are quite well, believe me, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. February 22, [1868 ?].

My dear Wallace,—I am hard at work on sexual selection and am driven half mad by the number of collateral points which require investigation, such as the relative number of the two sexes, and especially on polygamy. Can you aid me with respect to birds which have strongly marked secondary sexual characters, such as birds of paradise, humming-birds, the rupicola or rock-thrush, or any other such cases? Many gallinaeous birds certainly are polygamous. I suppose that birds may be known not to be polygamous if they are seen during the whole breeding season to associate in pairs, or if the male incubates, or aids in feeding the young. Will you have the kindness to turn this in your mind? but it is a shame to trouble you now that, as I am *heartily* glad to hear, you are at work on your Malayan travels. I am fearfully puzzled how far to extend your protective views with respect to the females in various classes. The more I work, the more important sexual selection apparently comes out.

Can butterflies be polygamous?—i.e. will one male impregnate more than one female?

Forgive me troubling you, and I daresay I shall have to ask your forgiveness again, and believe me, my dear Wallace, yours most sincerely,

CH. DARWIN.

P.S.—Baker has had the kindness to set the Entomological Society discussing the relative numbers of the sexes in insects, and has brought out some very curious results.

Is the orang polygamous? But I daresay I shall find that in your papers (I think) the *Annals and Magazine of Natural History*.

The following group of letters deals with the causes of the sterility of hybrids (*see* note in "More Letters," p. 287). Darwin's final view is given in the "Origin," 6th edit., 1900, p. 384. He acknowledges that it would be advantageous to two incipient species if, by physiological isolation due to mutual sterility, they could be kept from blending; but he continues: "After mature reflection, it seems to me that this could not have been effected through Natural Selection." And finally he concludes (p. 386): "But it would be superfluous to discuss this question

in detail; for with plants we have conclusive evidence that the sterility of crossed species must be due to some principle quite independent of Natural Selection. Both Gärtner and Kolreuter have proved that in genera including numerous species a series can be formed from species which, when crossed, yield fewer and fewer seeds, to species which never produce a single seed, but yet are affected by the pollen of certain other species, for the germen swells. It is here manifestly impossible to select the more sterile individuals, which have already ceased to yield seeds; so that this acme of sterility, when the germen alone is affected, cannot have been gained through selection; and from the laws governing the various grades of sterility being so uniform throughout the animal and vegetable kingdoms, we may infer that the cause, whatever it may be, is the same or nearly the same in all cases."

Wallace still adhered to his view (*see* "Darwinism," 1889, p. 174, *also* p. 292 of "More Letters," note I, and Letter 211, p. 299). The discussion of 1868 began with a letter from Wallace, written towards the end of February, giving his opinion on the "Variation of Animals and Plants"; the discussion on the sterility of hybrids is at p. 185, Vol. II., 1st edit.

(Second and third sheets of a letter from Wallace, apparently of February, 1868)

I am in the second volume of your book, and I have been astonished at the immense number of interesting facts you have brought together. I read the chapter on Pangenesis first, for I could not wait. I can hardly tell you how much I admire it. It is a positive *comfort* to me to have any feasible explanation of a difficulty that has always been haunting me, and I shall never be able to give it up till a better one supplies its place, and that I think hardly possible. You have now fairly beaten Spencer on his own ground, for he really offered no solution of the difficulties of the problem. The incomprehensible minuteness and vast numbers of the physiological germs or atoms (which themselves must be compounded of numbers of Spencer's physiological units) is the only difficulty, but that is only on a par with the difficulties in all conceptions of matter, space, motion, force, etc. As I understood Spencer, his physiological units were identical throughout each species, but slightly differ-

ent in each different species; but no attempt was made to show how the identical form of the parent or ancestors came to be built up of such units.

The only parts I have yet met with where I somewhat differ from your views, are in the chapter on the Causes of Variability, in which I think several of your arguments are unsound: but this is too long a subject to go into now.

Also, I do not see your objection to *sterility* between allied species having been aided by Natural Selection. It appears to me that, given a differentiation of a species into two forms each of which was adapted to a special sphere of existence, every slight degree of sterility would be a positive advantage, not to the *individuals* who were sterile, but to *each form*. If you work it out, and suppose the two incipient species, A, B, to be divided into two groups, one of which contains those which are fertile when the two are crossed, the other being slightly sterile, you will find that the latter will certainly supplant the former in the struggle for existence, remembering that you have shown that in such a cross the offspring would be *more vigorous* than the pure breed, and would therefore certainly soon supplant them, and as these would not be so well adapted to any special sphere of existence as the pure species A and B, they would certainly in their turn give way to A and B.

I am sure all naturalists will be disgusted at the malicious and ignorant article in the *Athenæum*. It is a disgrace to the paper, and I hope someone will publicly express the general opinion of it. We can expect no good reviews of your book till the quarterlies or best monthlies come out. I think the "Cambridge Man" of the "Darwinian Theory Examiner" must have written for the *Athenæum*. I shall be anxious to see how Pangenesis is received.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. February 27, 1868.

My dear Wallace,—You cannot well imagine how much I have been pleased by what you say about Pangenesis. None of my friends will speak out, except, to a certain extent, Sir H. Holland,¹ who found it very tough reading, but admits that some view

¹ Sir Henry Holland, Bart., M.D., F.R.S., a writer on Mental Physiology and other scientific subjects. B. 1788, D. 1873.

"closely akin to it" will have to be admitted. Hooker, as far as I understand him, which I hardly do at present, seems to think that the hypothesis is little more than saying that organisms have such and such potentialities. What you say exactly and fully expresses my feeling, viz. that it is a relief to have some feasible explanation of the various facts, which can be given up as soon as any better hypothesis is found. It has certainly been an immense relief to my mind; for I have been stumbling over the subject for years, dimly seeing that some relation existed between the various classes of facts. I now hear from H. Spencer that his views quoted in my footnote refer to something quite distinct, as you seem to have perceived.

I shall be very glad to hear, at some future day, your criticisms on the causes of variability.

Indeed, I feel sure that I am right about sterility and Natural Selection. Two of my grown-up children who are acute reasoners have two or three times at intervals tried to prove me wrong, and when your letter came they had another try, but ended by coming back to my side. I do not quite understand your case, and we think that a word or two is misplaced. I wish some time you would consider the case under the following point of view. If sterility is caused or accumulated through Natural Selection, then, as every degree exists up to absolute barrenness, Natural Selection must have the power of increasing it. Now take two species, A and B, and assume that they are (by any means) half-sterile, i.e. produce half the full number of offspring. Now try and make (by Natural Selection) A and B absolutely sterile when crossed, and you will find how difficult it is. I grant, indeed it is certain, that the degree of sterility of the individuals of A and B will vary, but any such extra-sterile individuals of, we will say, A, if they should hereafter breed with other individuals of A, will bequeath no advantage to their progeny, by which these families will tend to increase in number over other families of A, which are not more sterile when crossed with B. But I do not know that I have made this any clearer than in the chapter in my book. It is a most difficult bit of reasoning, which I have gone over and over again on paper with diagrams.

I shall be intensely curious to see your article in the *Journal of Travel*.

Many thanks for such answers as you could give. From what

you say I should have inferred that birds of paradise were probably polygamous. But after all, perhaps it is not so important as I thought. I have been going through the whole animal kingdom in reference to sexual selection, and I have just got to the beginning of Lepidoptera, i.e. to end of insects, and shall then pass on to Vertebrata. But my ladies next week are going (ill-luck to it) to take me nolens-volens to London for a whole month.

I suspect Owen wrote the article in the *Athenæum*, but I have been told that it is Berthold Seeman. The writer despises and hates me.

Hearty thanks for your letter—you have indeed pleased me, for I had given up the great god Pan as a still-born deity. I wish you could be induced to make it clear with your admirable powers of elucidation in one of the scientific journals.

I think we almost entirely agree about sexual selection, as I now follow you to large extent about protection to females, having always believed that colour was often transmitted to both sexes; but I do not go quite so far about protection.—Always yours most sincerely,

CH. DARWIN.

Hurstpierpoint. March 1, 1868.

My dear Darwin,—I beg to enclose what appears to me a demonstration, *on your own principles*, that Natural Selection *could produce sterility of hybrids*.

If it does not convince you I shall be glad if you will point out where the fallacy lies. I have taken the two cases of a slight sterility overcoming a perfect fertility, and of a perfect sterility overcoming a partial fertility—the beginning and end of the process. You admit that variations in fertility and sterility occur, and I think you will also admit that if I demonstrate that a considerable amount of sterility would be advantageous to a variety, that is sufficient proof that the slightest variation in that direction would be useful also, and would go on accumulating.

Sir C. Lyell spoke to me as if he greatly admired Pangenesis. I am very glad H. Spencer at once acknowledges that his view was something quite distinct from yours. Although, as you know, I am a great admirer of his, I feel how completely his view

failed to go to the root of the matter, as yours does. His explained nothing, though he was evidently struggling hard to find an explanation. Yours, as far as I can see, explains everything in *growth and reproduction*, though of course the mystery of *life and consciousness* remains as great as ever.

Parts of the chapter on Pangenesis I found hard reading, and have not quite mastered yet, and there are also throughout the discussions in Vol. II. many bits of hard reading on minute points which we, who have not worked experimentally at cultivation and crossing as you have done, can hardly see the importance of, or their bearing on the general question.

If I am asked, I may perhaps write an article on the book for some periodical, and if so shall do what I can to make Pangenesis appreciated.

I suppose Mrs. Darwin thinks you *must* have a holiday, after the enormous labour of bringing out such a book as that. I am sorry I am not now staying in town. I shall, however, be up for two days on Thursday, and shall hope to see you at the Linnean, where Mr. Trimen has a paper on some of his wonderful South African mimetic butterflies.

I hope this will reach you before you leave.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

Hurstpierpoint. March 8, 1868.

Dear Darwin,—I am very sorry your letter came back here while I was going to town, or I should have been very pleased to have seen you.

Trimen's paper at the Linnean was a very good one, but the only opponents were Andrew Murray and B. Seeman. The former talked utter nonsense about the "harmony of nature" produced by "polarisation," alike in "rocks, plants and animals," etc., etc., etc. And Seeman objected that there was mimicry among plants, and that our theory would not explain it.

Lubbock answered them both in his best manner.

Pray take your rest, and put my last notes by till you return to Down, or let your son discover the fallacies in them.

Would you like to see the specimens of pupæ of butterflies whose colours have changed in accordance with the colour of the

surrounding objects? They are very curious, and Mr. T. W. Wood, who bred them, would, I am sure, be delighted to bring them to show you. His address is 89 Stanhope Street, Hempstead Road, N.W.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

Darwin had already written a short note to Wallace expressing a general dissent from his views.

4 Chester Place, Regent's Park, N.W. March 17, 1868.

My dear Wallace,—Many thanks about *Pieridæ*. I have no photographs up here, but will remember to send one from Down. Should you care to have a large one, of treble or quadruple common size, I will with pleasure send you one under glass cover, to any address you like in London, either now or hereafter. I grieve to say we shall not be here on April 2nd, as we return home on the 31st. In summer I hope that Mrs. Wallace and yourself will pay us a visit at Down, soon after you return to London; for I am sure you will allow me the freedom of an invalid.

My paper to-morrow at the Linnean Society is simply to prove, alas! that primrose and cowslip are as good species as any in the world, and that there is no trustworthy evidence of one producing the other. The only interesting point is the frequency of the production of natural hybrids, i.e. oxlips, and the existence of one kind of oxlip which constitutes a third good and distinct species. I do not suppose that I shall be able to attend the Linnean Society to-morrow.

I have been working hard in collecting facts on sexual selection every morning in London, and have done a good deal; but the subject grows more and more complex, and in many respects more difficult and doubtful. I have had grand success this morning in tracing gradational steps by which the peacock tail has been developed: I quite feel as if I had seen a long line of its progenitors.

I do not feel that I shall grapple with the sterility argument till my return home; I have tried once or twice and it has made my stomach feel as if it had been placed in a vise. Your paper has driven three of my children half mad—one sat up to twelve o'clock over it. My second son, the mathematician, thinks that

you have omitted one almost inevitable deduction which apparently would modify the result. He has written out what he thinks, but I have not tried fully to understand him. I suppose that you do not care enough about the subject to like to see what he has written?

I hope your book progresses.

I am intensely anxious to see your paper in *Murray's Journal*.
—My dear Wallace, yours very sincerely, CH. DARWIN.

Hurstpierpoint. March 19, 1868.

Dear Darwin,—I should very much value a *large* photograph of you, and also a carte for my album, though it is too bad to ask you for both, as you must have so many applicants.

I am sorry I shall not see you in town, but shall look forward with pleasure to paying you a visit in the summer.

I am sorry about the Primulas, but I feel sure some such equally good case will some day be discovered, for it seems impossible to understand how all natural species whatever should have acquired sterility. Closely allied forms from adjacent islands would, I should think, offer the best chance of finding good species fertile “inter se”; since even if Natural Selection induces sterility I do not see how it could affect them, or why they should *always* be sterile, and varieties *never*.

I am glad you have got good materials on sexual selection. It is no doubt a difficult subject. One difficulty to me is, that I do not see how the constant *minute* variations, which are sufficient for Natural Selection to work with, could be *sexually* selected. We seem to require a series of bold and abrupt variations. How can we imagine that an inch in the tail of a peacock, or a quarter of an inch in that of the bird of paradise, would be noticed and preferred by the female?

Pray let me see what your son says about the sterility selection question. I am deeply interested in all that concerns the powers of Natural Selection, but though I admit there are a few things it cannot do I do not yet believe sterility to be one of them.

In case your son has turned his attention to mathematical physics, will you ask him to look at the enclosed question, which I have vainly attempted to get an answer to?—Believe me, yours very faithfully,

ALFRED R. WALLACE.

4 *Chester Place, Regent's Park, N.W.* March 19-24, 1868.

My dear Wallace,—I have sent your query to Cambridge to my son. He ought to answer it, for he got his place of Second Wrangler chiefly by solving very difficult problems. I enclose his remarks on two of your paragraphs: I should like them returned some time, for I have not studied them, and let me have your impression.

I have told E. Edwards to send one of my large photographs to you addressed to 76½ Westbourne Grove, not to be forwarded. When at home I will send my carte.

The sterility is a most puzzling problem. I can see so far, but I am hardly willing to admit all your assumptions, and even if they were all admitted, the process is so complex and the sterility (as you remark in your note) so universal, even with species inhabiting quite distinct countries (as I remarked in my chapter), together with the frequency of a difference in reciprocal unions, that I cannot persuade myself that it has been gained by Natural Selection, any more than the difficulty of grafting distinct genera and the impossibility of grafting distinct families. You will allow, I suppose, that the capacity of grafting has not been directly acquired through Natural Selection.

I think that you will be pleased with the second volume or part of Lyell's Principles, just out.

In regard to sexual selection. A girl sees a handsome man, and without observing whether his nose or whiskers are the tenth of an inch longer or shorter than in some other man, admires his appearance and says she will marry him. So, I suppose, with the pea-hen; and the tail has been increased in length merely by, on the whole, presenting a more gorgeous appearance. Jenner Weir, however, has given me some facts showing that birds apparently admire details of plumage.—Yours most sincerely,

C. DARWIN.

Hurstpierpoint. March 24, [1868 ?].

Dear Darwin,—Many thanks for the photo, which I shall get when I go to town.

I return your son's notes with my notes on them.

Without going into any details, is not this a strong general argument?—

1. A species varies occasionally in two directions, but owing

to their free intercrossing they (the variations) never increase.

2. A change of conditions occurs which threatens the existence of the species, but the *two varieties* are adapted to the changing conditions, and, if accumulated, will form two new *species adapted to the new conditions*.

3. Free crossing, however, renders this impossible, and so the species is in danger of extinction.

4. If *sterility* could be induced, then the pure races would increase more rapidly and replace the old species.

5. It is admitted that *partial sterility* between *varieties* does occasionally occur. It is admitted the *degree* of this *sterility varies*. Is it not probable that Natural Selection can accumulate these variations and thus save the species?

If Natural Selection can *not* do this, how do species ever arise, except when a variety is isolated?

Closely allied species in distinct countries being sterile is no difficulty, for either they diverged from a common ancestor in contact, and Natural Selection increased the sterility, or they were isolated, and have varied since, in which case they have been for ages influenced by distinct conditions which may well produce sterility.

If the difficulty of *grafting* was as great as the difficulty of *crossing*, and as *regular*, I admit it would be a most serious objection. But it is not. I believe many distinct species can be grafted while others less distinct cannot. The regularity with which natural (?) species are sterile together, even when *very much alike*, I think is an argument in favour of the sterility having been generally produced by Natural Selection for the good of the species.

The other difficulty, of unequal sterility of reciprocal crosses, seems none to me; for it is a step to more complete sterility, and as such would be useful and would be increased by selection.

I have read Sir C. Lyell's second volume with great pleasure. He is, as usual, very cautious, and hardly ever expresses a positive opinion, but the general effect of the whole book is very strong, as the argument is all on our side.

I am in hopes it will bring in a new set of converts to Natural Selection, and will at all events lead to a fresh ventilation of the subject.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

• 4 *Chester Place, Regent's Park, N.W.* March 27, 1868.

My dear Wallace,—My son has failed in your problem, and says that it is "excessively difficult": he says you will find something about it in Thomson and Tait, *Natural Philosophy* (art. 649). He has, however, sent the solution, if the plate rested on a square rim, but he supposes this will not answer your purpose; nevertheless, I have forwarded it by this same post. It seems that the rim being round makes the problem much more difficult.

I enclose my photograph, which I have received from Down. I sent your answer to George on his objection to your argument on sterility, but have not yet heard from him. I dread beginning to think over this fearful problem, which I believe beats the plate on the circular rim; but I will sometime. I foresee, however, that there are so many doubtful points that we shall never agree. As far as a glance serves it seems to me, perhaps falsely, that you sometimes argue that hybrids have an advantage from greater vigour, and sometimes a disadvantage from not being so well fitted to their conditions. Heaven protect my stomach whenever I attempt following your argument.
—Yours most sincerely, C. DARWIN.

Down, Bromley, Kent. April 6, 1868.

My dear Wallace,—I have been considering the terrible problem. Let me first say that no man could have more earnestly wished for the success of Natural Selection in regard to sterility than I did, and when I considered a general statement (as in your last note) I always felt sure it could be worked out, but always failed in detail, the cause being, as I believe, that Natural Selection cannot effect what is not good for the individual, including in this term a social community. It would take a volume to discuss all the points; and nothing is so humiliating to me as to agree with a man like you (or Hooker) on the premises and disagree about the result.

I agree with my son's argument and not with rejoinder. The cause of our difference, I think, is that I look at the number of offspring as an important element (all circumstances remaining the same) in keeping up the average number of individuals within any area. I do not believe that the amount of food by any means is the sole determining cause of number. Lessened

fertility is equivalent to a new source of destruction. I believe if in one district a species produce *from any cause* fewer young, the deficiency would be supplied from surrounding districts. This applies to your par. (5). If the species produced fewer young from any cause in *every* district, it would become extinct unless its fertility were augmented through Natural Selection (*see* H. Spencer).

I demur to probability and almost to possibility of par. (1), as you start with two forms, within the same area, which are not mutually sterile, and which yet have supplanted the parent-form (par. 6). I know of no ghost of a fact supporting belief that disinclination to cross accompanies sterility. It cannot hold with plants, or the lower fixed aquatic animals. I saw clearly what an immense aid this would be, but gave it up. Disinclination to cross seems to have been independently acquired, probably by Natural Selection; and I do not see why it would not have sufficed to have prevented incipient species from blending to have simply increased sexual disinclination to cross.

Par. (11): I demur to a certain extent to amount of sterility and structural dissimilarity necessarily going together, except indirectly and by no means strictly. Look at the case of pigeons, fowls, and cabbages.

I overlooked the advantage of the half-sterility of reciprocal crosses; yet, perhaps from novelty, I do not feel inclined to admit the probability of Natural Selection having done its work so clearly.

I will not discuss the second case of utter sterility; but your assumptions in par. 13 seem to me much too complicated. I cannot believe so universal an attribute as utter sterility between remote species was acquired in so complex a manner. I do not agree with your rejoinder on grafting; I fully admit that it is not so closely restricted as crossing; but this does not seem to me to weaken the case as one of analogy. The incapacity of grafting is likewise an invariable attribute of plants sufficiently remote from each other, and sometimes of plants pretty closely allied.

The difficulty of increasing the sterility, through Natural Selection, of two already sterile species seems to me best brought home by considering an actual case. The cowslip and primrose are moderately sterile, yet occasionally produce hybrids: now

these hybrids, two or three or a dozen in a whole parish, occupy ground which *might* have been occupied by either pure species, and no doubt the latter suffer to this small extent. But can you conceive that any individual plants of the primrose and cowslip, which happened to be mutually rather more sterile (i.e. which when crossed yielded a few less seed) than usual, would profit to such a degree as to increase in number to the ultimate exclusion of the present primrose and cowslip? I cannot.

My son, I am sorry to say, cannot see the full force of your rejoinder in regard to the second head of continually augmented sterility. You speak in this rejoinder, and in par. (5) of all the individuals becoming in some slight degree sterile in certain districts; if you were to admit that by continued exposure to these same conditions the sterility would inevitably increase, there would be no need of Natural Selection. But I suspect that the sterility is not caused so much by any particular conditions, as by long habituation to conditions of any kind. To speak according to Pangenesis, the gemmules of hybrids are not injured, for hybrids propagate freely by buds; but their reproductive organs are somehow affected, so that they cannot accumulate the proper gemmules in nearly the same manner as the reproductive organs of a pure species become affected when exposed to unnatural conditions.

This is a very ill-expressed and ill-written letter. Do not answer it, unless the spirit urges you. Life is too short for so long a discussion. We shall, I *greatly* fear, never agree.—My dear Wallace, most sincerely yours,

CH. DARWIN.

Hurstpierpoint. [?] April 8, 1868.

Dear Darwin,—I am sorry you should have given yourself the trouble to answer my ideas on Sterility. If you are not convinced, I have little doubt but that I am wrong; and in fact I was only *half convinced* by my own arguments, and I now think there is about an even chance that Natural Selection may or not be able to accumulate sterility. If my first proposition is modified to *the existence of a species and a variety in the same area*, it will do just as well for my argument. Such certainly do exist. They are fertile together, and yet each maintains itself tolerably distinct. How can this be, if there is no disinclination

to crossing? My belief certainly is that number of offspring is not so important an element in keeping up population of a species as supply of food and other favourable conditions, because the numbers of a species constantly vary greatly in different parts of its area, whereas the average number of offspring is not a very variable element.

However, I will say no more but leave the problem as insoluble, only fearing that it will become a formidable weapon in the hands of the enemies of Natural Selection.

While writing a few pages on the northern Alpine forms of plants on the Java mountains I wanted a few cases to refer to like Teneriffe, where there are no *northern* forms, and scarcely any alpine. I expected the volcanoes of Hawaii would be a good case, and asked Dr. Seeman about them. It seems a man has lately published a list of Hawaiian plants, and the mountains swarm with European alpine genera and some species!¹ Is not this most extraordinary and a puzzler? They are, I believe, truly oceanic islands in the absence of mammals and the extreme poverty of birds and insects, and they are within the tropics. Will not that be a hard nut for you when you come to treat in detail on geographical distribution?

I enclose Seeman's note, which please return when you have copied the list, if of any use to you.

Many thanks for your carte, which I think very good. The large one had not arrived when I was in town last week.

Sir C. Lyell's chapter on Oceanic Islands I think very good. —Believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. April 9, 1868.

My dear Wallace,—You allude in your note to several points which I should much enjoy discussing with you did time and strength permit. I know Dr. Seeman is a good botanist, but I most strongly advise you to show the list to Hooker before you make use of the materials in print. Hooker seems much overworked, and is now gone a tour, but I supposed you will be in town before very long, and could see him. The list is quite

¹ "This turns out to be inaccurate, or greatly exaggerated. There are no true alpine, and the European genera are comparatively few. See my 'Island Life,' p. 323."—A. R. W.

unintelligible to me; it is not pretended that the same species exist in the Sandwich Islands and Arctic regions; and as far as the genera are concerned, I know that in almost every one of them species inhabit such countries as Florida, North Africa, New Holland, etc. Therefore these genera seem to me almost mundane, and their presence in the Sandwich Islands will not, as I suspect in my ignorance, show any relation to the Arctic regions. The Sandwich Islands, though I have never considered them much, have long been a sore perplexity to me; they are eminently oceanic in position and production; they have long been separated from each other; and there are only slight signs of subsidence in the islets to the westward. I remember, however, speculating that there must have been some immigration during the glacial period from North America or Japan; but I cannot remember what my grounds were. Some of the plants, I think, show an affinity with Australia. I am very glad that you like Lyell's chapter on Oceanic Islands, for I thought it one of the best in the part which I have read. If you do not receive the big photo of me in due time, let me hear.—Yours very sincerely,

CH. DARWIN.

The following refers to Wallace's article, "A Theory of Birds' Nests," in Andrew Murray's *Journal of Travel*, i. 73. He here treats in fuller detail the view already published in the *Westminster Review* for July, 1867, p. 38. The rule which Wallace believes, with very few exceptions, to hold good is, "that when both sexes are of strikingly gay and conspicuous colours, the nest is . . . such as to conceal the sitting bird; while, whenever there is a striking contrast of colours, the male being gay and conspicuous, the female dull and obscure, the nest is open and the sitting bird exposed to view." At this time Wallace allowed considerably more influence to *sexual* selection (in combination with the need of protection) than in his later writings. See his letter to Darwin of July 23, 1877 (p. 244), which fixes the period at which the change in his views occurred. He finally rejected Darwin's theory that colours "have been developed by the preference of the females, the more ornamented males becoming the parents of each successive generation." (See "Darwinism," 1889, p. 285.

Down, Bromley, Kent, S.E. April 15, 1868.

My dear Wallace,—I have been deeply interested by your admirable article on Birds' Nests. I am delighted to see that we really differ very little—not more than two men almost always will. You do not lay much or any stress on new characters spontaneously appearing in one sex (generally the male) and being transmitted exclusively, or more commonly only in excess, to that sex. I, on the other hand, formerly paid far too little attention to protection. I had only a glimpse of the truth. But even now I do not go quite as far as you. I cannot avoid thinking rather more than you do about the exceptions in nesting to the rule, especially the partial exceptions, i.e. when there is some little difference between the sexes in species which build concealed nests. I am now quite satisfied about the incubating males; there is so little difference in conspicuousness between the sexes. I wish with all my heart I could go the whole length with you. You seem to think that such birds probably select the most beautiful females: I must feel some doubt on this head, for I can find no evidence of it. Though I am writing so carping a note, I admire the article *thoroughly*.

And now I want to ask a question. When female butterflies are more brilliant than their males, you believe that they have in most cases, or in all cases, been rendered brilliant so as to mimic some other species and thus escape danger. But can you account for the males not having been rendered equally brilliant and equally protected? Although it may be most for the welfare of the species that the female should be protected, yet it would be some advantage, certainly no disadvantage, for the unfortunate male to enjoy an equal immunity from danger. For my part, I should say that the female alone had happened to vary in the right manner, and that the beneficial variations had been transmitted to the same sex alone. Believing in this, I can see no improbability (but from analogy of domestic animals a strong probability): the variations leading to beauty must *often* have occurred in the males alone, and been transmitted to that sex alone. Thus I should account in many cases for the greater beauty of the male over the female, without the need of the protective principle. I should be grateful for an answer on this point.

I hope that your Eastern book progresses well.—My dear Wallace, yours sincerely,

C. DARWIN.

Sir Clifford Allbutt's view, referred to in the following letter, probably had reference to the fact that the sperm-cell goes, or is carried, to the germ-cell, never vice versa. In this letter Darwin gives the reason for the "law" referred to. Wallace has been good enough to supply the following note [May 27, 1902]: "It was at this time that my paper on 'Protective Resemblance' first appeared in the *Westminster Review*, in which I adduced the greater, or, rather, the more continuous, importance of the female (in the lower animals) for the race, and my 'Theory of Birds' Nests' (*Journal of Travel and Natural History*, No. 2) in which I applied this to the usually dull colours of female butterflies and birds. It is to these articles, as well as to my letters, that Darwin chiefly refers."

Down, Bromley, Kent, S.E. April 30, 1868.

My dear Wallace,—Your letter, like so many previous ones, has interested me much. Dr. Allbutt's view occurred to me some time ago, and I have written a short discussion on it. It is, I think, a remarkable law, to which I have found no exception. The foundation lies in the fact that in many cases the eggs or seeds require nourishment and protection by the mother-form for some time after impregnation. Hence the spermatozoa and antherozoids travel in the lower aquatic animals and plants to the female, and pollen is borne to the female organ. As organisms rise in the scale it seems natural that the male should carry the spermatozoa to the females in his own body. As the male is the searcher he has received and gained more eager passions than the female; and, very differently from you, I look at this as *one* great difficulty in believing that the males select the more attractive females; as far as I can discover they are always ready to seize on any female, and sometimes on many females. Nothing would please me more than to find evidence of males selecting the more attractive females [¹ in pigeons¹]: I have for months been trying to persuade myself of this. There

¹ The words "in pigeons" and "lizards" enclosed in brackets were inserted by Wallace.

is the case of man in favour of this belief, and I know in hybrid [*lizards*¹] unions of males preferring particular females, but alas! not guided by colour. Perhaps I may get more evidence as I wade through my twenty years' mass of notes.

I am not shaken about the female protected butterflies: I will grant (only for argument) that the life of the male is of *very* little value; I will grant that the males do not vary; yet why has not the protective beauty of the female been transferred by inheritance to the male? The beauty would be a gain to the male, as far as we can see, as a protection; and I cannot believe that it would be repulsive to the female as she became beautiful. But we shall never convince each other. I sometimes marvel how truth progresses, so difficult is it for one man to convince another unless his mind is vacant. Nevertheless, I myself to a certain extent contradict my own remarks; for I believe *far more* in the importance of protection than I did before reading your articles.

I do not think you lay nearly stress enough in your articles on what you admit in your letter, viz. "there seems to be some production of vividness . . . of colour in the male independent of protection." This I am making a chief point; and have come to your conclusion so far that I believe that intense colouring in the female sex is often checked by being dangerous.

That is an excellent remark of yours about no known case of the male *alone* assuming protective colours; but in the cases in which protection has been gained by dull colours, I presume that sexual selection would interfere with the male losing his beauty. If the male alone had acquired beauty as a protection, it would be most readily overlooked, as males are so often more beautiful than their females. Moreover, I grant that the loss of the male is somewhat less precious and thus there would be less rigorous selection with the male, so he would be less likely to be made beautiful through Natural Selection for protection. (This does not apply to sexual selection, for the greater the excess of males and the less precious their lives, so much the better for sexual selection.) But it seems to me a good argument, and very good if it could be thoroughly established.—
Yours most sincerely,

C. DARWIN.

I do not know whether you will care to read this scrawl.

P.S.—I heard yesterday that my photograph had been sent to your London address—Westbourne Grove.

Down, Bromley, Kent, S.E. May 5, 1868.

My dear Wallace,—I am afraid I have caused you a great deal of trouble in writing to me at such length. I am glad to say that I agree almost entirely with your summary, except that I should put sexual selection as an equal, or perhaps as even a more important agent in giving colour than natural selection for protection. As I get on in my work I hope to get clearer and more decided ideas. Working up from the bottom of the scale I have as yet only got to fishes. What I rather object to in your articles is that I do not think any one would infer from them that you place sexual selection even as high as No. 4 in your summary. It was very natural that you should give only a line to sexual selection in the summary to the *Westminster Review*, but the result at first to my mind was that you attributed hardly anything to its power. In your penultimate note you say: "In the great mass of cases in which there is *great* differentiation of colour between the sexes, I believe it is due *almost wholly* to the need of protection to the female." Now, looking to the whole animal kingdom I can at present by no means admit this view; but pray do not suppose that because I differ to a certain extent, I do not thoroughly admire your several papers and your admirable generalisation on birds' nests. With respect to this latter point, however, although following you, I suspect that I shall ultimately look at the whole case from a rather different point of view.

You ask what I think about the gay-coloured females of *Pieris*:¹ I believe I quite follow you in believing that the colours are wholly due to mimicry; and I further believe that the male is not brilliant from not having received through inheritance colour from the female, and from not himself having varied; in short, that he has not been influenced by Selection.

I can make no answer with respect to the elephants. With respect to the female reindeer, I have hitherto looked at the horns simply as the consequence of inheritance *not* having been limited by sex.

¹ See *Westminster Review*, July, 1867, p. 37.

Your idea about colour being concentrated in the smaller males seems good, and I presume that you will not object to my giving it as your suggestion.—Believe me, my dear Wallace, with many thanks, yours very sincerely,

CH. DARWIN.

Wallace's more recent views on the question of Natural Selection and Sterility may be found in a note written by him in 1889: "When writing my 'Darwinism' and coming again to the consideration of the problem of the effect of Natural Selection in accumulating variations in the amount of sterility between varieties or incipient species, twenty years later, I became more convinced than I was when discussing with Darwin of the substantial accuracy of my argument. Recently a correspondent who is both a naturalist and a mathematician has pointed out to me a slight error in my calculation at p. 183 (which does not, however, materially affect the result), disproving the physiological selection of the late Dr. Romanes, but he can see no fallacy in my argument as to the power of Natural Selection to increase sterility between incipient species, nor, so far as I am aware, has anyone shown such fallacy to exist.

"On the other points on which I differed from Mr. Darwin in the foregoing discussion—the effect of high fertility on population of a species, etc.—I still hold the views I then expressed, but it would be out of place to attempt to justify them here."—A. R. W.

9 St. Mark's Crescent, N.W. August 16, [1868?].

Dear Darwin,—I ought to have written before to thank you for the copies of your paper on "Primula" and on "Cross Unions of Dimorphic Plants, etc." The latter is particularly interesting, and the conclusion most important; but I think it makes the difficulty of *how* these forms, with their varying degrees of sterility, originated, greater than ever. If Natural Selection could not accumulate varying degrees of sterility for the plants' benefit, then how did sterility ever come to be associated with *one* cross of a trimorphic plant rather than another? The difficulty seems to be increased by the consideration that the advantage of a cross with a *distinct individual* is gained just as well by illegitimate as by legitimate unions. By what means,

then, did illegitimate unions ever become sterile? It would seem a far simpler way for each plant's pollen to have acquired a prepotency on another individual's stigma over that of the same individual, without the extraordinary complication of three differences of structure and eighteen different unions with varying degrees of sterility!

However, the fact remains an excellent answer to the statement that sterility of hybrids proves the absolute distinctness of the parents.

I have been reading with great pleasure Mr. Bentham's last admirable address,¹ in which he so well replies to the gross misstatements of the *Athenæum*; and also says a word in favour of Pangenesis. I think we may now congratulate you on having made a valuable convert, whose opinions on the subject, coming so late and being evidently so well considered, will have much weight.

I am going to Norwich on Tuesday to hear Dr. Hooker, who I hope will boldly promulgate "Darwinianism" in his address. Shall we have the pleasure of seeing you there?

I am engaged in negotiations about my book.

Hoping you are well and getting on with your next volumes.—Believe me, yours very faithfully,

ALFRED R. WALLACE.

Freshwater, Isle of Wight. August 19, 1868.

My dear Wallace,—Thanks for your note. I did sometimes think of going to Norwich, for I should have very much liked it, but it has been quite out of the question. We have been here for five weeks for a change, and it has done me some little good; but I have been forced to live the life of a drone, and for a month before leaving home I was unable to do anything and had to stop all work.

We return to Down to-morrow.

Hooker has been here for two or three days, so that I have had much talk about his Address. I am glad that you will be there.

It is real good news that your book is so advanced that you are negotiating about its publication.

¹ *Proc. Linn. Soc.*, 1867-8, p. 57.

With respect to dimorphic plants: it is a great puzzle, but I *fancy* I partially see my way—too long for a letter and too speculative for publication. The groundwork of the acquirement of such peculiar fertility (for what you say about any other distinct individual being, as it would appear, sufficient, is very true) rests on the stamens and pistil having varied first in relative length, *as actually occurs* irrespective of dimorphism, and the peculiar kind of fertility characteristic of dimorphic and the trimorphic plants having been *secondarily* acquired. Pangenesis makes *very* few converts: G. H. Lewes is one.

I had become, before my nine weeks' horrid interruption of all work, extremely interested in sexual selection and was making fair progress. In truth, it has vexed me much to find that the further I get on, the more I differ from you about the females being dull-coloured for protection. I can now hardly express myself as strongly even as in the "Origin." This has *much decreased* the pleasure of my work.

In the course of September, if I can get at all stronger, I hope to get Mr. J. Jenner Weir (who has been *wonderfully* kind in giving me information) to pay me a visit, and I will then write for the chance of your being able to come and, I hope, bring with you Mrs. Wallace. If I could get several of you together, it would be less dull for you, for of late I have found it impossible to talk with any human being for more than half an hour, except on extraordinarily good days.—Believe me, my dear Wallace, ever yours sincerely,

CH. DARWIN.

9 St. Mark's Crescent. August 30, [1868 ?].

Dear Darwin,—I was very sorry to hear you had been so unwell again, and hope you will not exert yourself to write me such long letters. Darwinianism was in the ascendant at Norwich (I hope you do not dislike the word, for we really *must* use it), and I think it rather disgusted some of the parsons, joined with the amount of *advice* they received from Hooker and Huxley. The worst of it is that there are no opponents left who know anything of natural history, so that there are none of the good discussions we used to have. G. H. Lewes seems to me to be making a great mistake in the *Fortnightly*, advocating *many distinct* origins for different groups, and even, if I understand him, distinct origins for some allied groups, just as the

anthropologists do who make the red man descend from the orang, the black man from the chimpanzee—or rather the Malay and orang one ancestor, the negro and chimpanzee another. Vogt told me that the Germans are all becoming converted by your last book.

I am certainly surprised that you should find so much evidence against protection having checked the acquirement of bright colour in females; but I console myself by presumptuously hoping that I can explain your facts, unless they are derived from the very groups on which I chiefly rest—birds and insects. There is nothing *necessarily* requiring protection in females; it is a matter of habits. There are groups in which both sexes require protection in an exactly equal degree, and others (I think) in which the male requires most protection, and I feel the greatest confidence that these will ultimately support my view, although I do *not* yet know the facts they may afford.

Hoping you are in better health, believe me, dear Darwin,
yours faithfully,

ALFRED R. WALLACE.

9 St. Mark's Crescent, N.W. September 5, [1868 ?].

Dear Darwin,—It will give me great pleasure to accept your kind invitation for next Saturday and Sunday, and my wife would very much like to come too, and will if possible. Unfortunately, there is a new servant coming that very day, and there is a baby at the mischievous age of a year and a quarter to be left in somebody's care; but I daresay it will be managed somehow.

I will drop a line on Friday to say if we are coming the time you mention.—Believe me yours very faithfully,

ALFRED R. WALLACE.

Friday.

My dear Darwin,—My wife has arranged to accompany me to-morrow, and we hope to be at Orpington Station at 5.44, as mentioned by you.—Very truly yours,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. September 16, 1868.

My dear Wallace,—The beetles have arrived, and cordial thanks: I never saw such wonderful creatures in my life. I was

thinking of something quite different. I shall wait till my son Frank returns, before soaking and examining them. I long to steal the box, but return it by this post, like a too honest man.

I am so much pleased about the male musk *Callichroma*; for by odd chance I told Frank a week ago that next spring he must collect at Cambridge lots of *Cerambyx moschatus*, for as sure as life he would find the odour sexual!

You will be pleased to hear that I am undergoing severe distress about protection and sexual selection: this morning I oscillated with joy towards you; this evening I have swung back to the old position, out of which I fear I shall never get.

I did most thoroughly enjoy my talk with you three gentlemen, and especially with you, and to my great surprise it has not knocked me up. Pray give my kindest remembrances to Mrs. Wallace, and if my wife were at home she would cordially join in this.—Yours very sincerely,
CH. DARWIN.

I have had this morning a capital letter from Walsh of Illinois; but details too long to give.

Among Wallace's papers was found the following draft of a letter of his to Darwin:

9 St. Mark's Crescent, N.W. September 18, 1868.

Dear Darwin,—The more I think of your views as to the colours of females, the more difficulty I find in accepting them, and as you are now working at the subject I hope it will not interrupt you to hear "counsel on the other side."

I have a "general" and a "special" argument to submit.

1. Female birds and insects are generally exposed to more danger than the male, and in the case of insects their existence is necessary for a longer period.

2. They therefore require in some way or other a special balance of protection.

3. Now, if the male and female were distinct species, with different habits and organisations, you would, I think, at once admit that a difference of colour serving to make that one less conspicuous which evidently required more protection than the other had been acquired by Natural Selection.

4. But you admit that variations appearing in one sex are transmitted (often) to that sex only: there is therefore nothing to prevent Natural Selection acting on the two sexes as if they were two species.

5. Your objection that the same protection would to a certain extent be useful to the male, seems to me utterly unsound, and directly opposed to your own doctrine so convincingly urged in the "Origin," "*that Natural Selection never can improve an animal beyond its needs.*" So that admitting abundant variation of colour in the male, it is impossible that he can be brought by Natural Selection to resemble the female (unless *her* variations are always transmitted to *him*) because the *difference* of their colours is to balance the *difference* in their organisations and habits, and Natural Selection cannot *give* to the male *more* than is needed to effect that balance.

6. The fact that in almost all protected groups the females perfectly resemble the males shows, I think, a tendency to transference of colour from one sex to the other when this tendency is not injurious.

Or perhaps the *protection* is acquired because this tendency exists. I admit therefore in the case of concealed nests they [habits] may have been acquired for protection.

Now for the special case.

7. In the very weak-flying *Leptalis* both sexes mimic *Heli-conidæ*.

8. In the much more powerful *Papilio*, *Pieris*, and *Diadema* it is generally the *female only* that mimics *Danais*.

9. In these cases the females often acquire more bright and varied colours than the male. Sometimes, as in *Pieris pyrrha*, conspicuously so.

10. No single case is known of a male *Papilio*, *Pieris*, *Diadema* (or any other insect?) *alone* mimicking a *Danais*, etc.

11. But colour is more frequent in males, and *variations* always seem ready for purposes of sexual or other selection.

12. The fair inference seems to be that given in proposition 5 of the general argument, viz. that *each species* and *each sex* can only be modified by selection just as far as is absolutely necessary, not a step farther. A male, being by structure and habits less exposed to danger and less requiring protection than the female, cannot have more protection given to it by Natural

Selection, but a female must have some extra protection to balance the greater danger, and she rapidly acquires it in one way or another.

13. An objection derived from cases like male fish, which seem to require protection, yet having brighter colours, seems to me of no more weight than is that of the existence of many white and unprotected species of *Leptalis* to Bates's theory of mimicry, that only one or two species of butterflies perfectly resemble leaves, or that the instincts or habits or colours that seem essential to the preservation of one animal are often totally absent in an allied species.

Down, Bromley, Kent. September 23, 1868.

My dear Wallace,—I am very much obliged for all your trouble in writing me your long letter, which I will keep by me and ponder over. To answer it would require at least 200 folio pages! If you could see how often I have rewritten some pages, you would know how anxious I am to arrive as near as I can to the truth. We differ, I think, chiefly from fixing our minds perhaps too closely on different points, on which we agree: I lay great stress on what I know takes place under domestication: I think we start with different fundamental notions on inheritance. I find it most difficult, but not, I think, impossible, to see how, for instance, a few red feathers appearing on the head of a male bird, and which *are at first transmitted to both sexes*, could come to be transmitted to males alone;¹ but I have no difficulty in making the whole head red if the few red feathers in the male from the first tended to be sexually transmitted. I am quite willing to admit that the female may have been modified, either at the same time or subsequently, for protection, by the accumulation of variations limited in their transmission to the female sex. I owe to your writings the consideration of this latter point. But I cannot yet persuade myself that females *alone* have often been modified for protection.

¹ It is not enough that females should be produced from the males with red feathers, which should be destitute of red feathers; but these females must have a *latent tendency* to produce such feathers, otherwise they would cause deterioration in the red head-feathers of their male offspring. Such latent tendency would be shown by their producing the red feathers when old or diseased in their ovaria.

Should you grudge the trouble briefly to tell me whether you believe that the plainer head and less bright colours of ♀¹ chaffinch, the less red on the head and less clean colours of ♀ goldfinch, the much less red on breast of ♀ bullfinch, the paler crest of goldencrest wren, etc., have been acquired by them for protection? I cannot think so; any more than I can that the considerable differences between ♀ and ♂ house-sparrow or much greater brightness of ♂ *Parus cæruleus* (both of which build under cover) than of ♀ *Parus* are related to protection. I even misdoubt much whether less blackness of blackbird is for protection.

Again, can you give me reason for believing that the merest differences between female pheasants, the female *Gallus bankiva*, the female of black grouse, the peahen, female partridge, have all special reference to protection under slightly different conditions? I of course admit that they are all protected by dull colours, derived, as I think, from some dull-ground progenitor; and I account partly for their difference by partial transference of colour from the male, and by other means too long to specify; but I earnestly wish to see reason to believe that each is specially adapted for concealment to its environment.

I grieve to differ from you, and it actually terrifies me, and makes me constantly distrust myself.

I fear we shall never quite understand each other. I value the cases of bright-coloured, incubating male fishes—and brilliant female butterflies, solely as showing that one sex may be made brilliant without any necessary transference of beauty to the other sex; for in these cases I cannot suppose that beauty in the other sex was checked by selection.

I fear this letter will trouble you to read it. A very short answer about your belief in regard to the ♀ finches and Gallinaceæ would suffice.—Believe me, my dear Wallace, yours very sincerely,

CH. DARWIN.

9 St. Mark's Crescent, N.W. September 27, 1868.

Dear Darwin,—Your view seems to be that variations occurring in one sex are transmitted either to that sex exclusively or to both sexes equally, or more rarely partially transferred.

¹ The symbols ♂, ♀ stand for male and female.

But we have every gradation of sexual colours from total dissimilarity to perfect identity. If this is explained solely by the laws of inheritance, then the colours of one or other sex will be always (in relation to their environment) a *matter of chance*. I cannot think this. I think Selection more powerful than laws of inheritance of which it makes use, as shown by cases of two, three or four forms of female butterflies, all of which have, I have little doubt, been specialised for protection.

To answer your first question is most difficult, if not impossible, because we have no sufficient evidence in *individual cases of slight sexual difference*, to determine whether the male alone has acquired his superior brightness by sexual selection, or the female been made duller by need of protection, or whether the two causes have acted. Many of the sexual differences of existing species may be inherited differences from parent forms who existed under different conditions and had greater or less need of protection.

I think I admitted before the general tendency (probably) of males to acquire brighter tints. Yet this cannot be universal, for many female birds and quadrupeds have equally bright tints.

I think the case of ♀ *Pieris pyrrha* proves that females alone can be greatly modified for protection.

To your second question I can reply more decidedly. I do think the females of the Gallinaceæ you mention have been modified or been prevented from acquiring the brighter plumage of the male by need of protection. I know that the *Gallus bankiva* frequents drier and more open situations than the peahen of Java, which is found among grassy and leafy vegetation corresponding with the colours of the two. So the Argus pheasant, ♂ and ♀, are, I feel sure, protected by their tints corresponding to the dead leaves of the lofty forest in which they dwell, and the female of the gorgeous fire-back pheasant, *Lophura viellottii*, is of a very similar *rich brown colour*. I do not, however, at all think the question can be settled by individual cases, but only by large masses of facts.

The colours of the mass of female birds seem to me strictly analogous to the colours of both sexes of snipes, woodcocks, plovers, etc., which are undoubtedly protective.

Now, supposing, on your view, that the colours of a male

bird become more and more brilliant by sexual selection, and a good deal of that colour is transmitted to the female till it becomes positively injurious to her during incubation and the race is in danger of extinction, do you not think that all the females who had acquired less of the male's bright colours or who themselves varied in a protective direction would be preserved, and that thus a good protective colouring would be acquired? If you admit that this could occur, and can show no good reason why it should not often occur, then we no longer differ, for this is the main point of my view.

Have you ever thought of the red wax-tips of the *Bombycilla* beautifully imitating the red fructifications of lichens used in the nest, and therefore the females have it too? Yet this is a very sexual-looking character.

We begin printing this week.—Yours very faithfully,

ALFRED R. WALLACE.

P.S.—Pray don't distress yourself on this subject. It will all come right in the end, and after all it is only an episode in your great work.—A. R. W.

9 St. Mark's Crescent, N.W. October 4, 1868.

Dear Darwin,—I should have answered your letter before, but have been very busy reading over my MSS. the last time before going to press, drawing maps, etc., etc.

Your first question cannot be answered, because we have not, in *individual cases of slight sexual* difference, sufficient evidence to determine how much of that difference is due to sexual selection acting on the male, how much to natural selection (protective) acting on the female, or how much of the difference may be due to inherited differences from ancestors who lived under different conditions. On your second question I can give an opinion. I do think the females of the Gallinaceæ you mention have been either *modified*, or *prevented from acquiring much of the brighter plumage of the males*, by the need of protection. I know that *Gallus bankiva* frequents drier and more open situations than *Pavo muticus*, which in Java is found among grassy and leafy vegetation corresponding with the colours of the two females. So the Argus pheasants, male and female, are, I feel sure, protected by their tints corresponding to dead

leaves of the dry lofty forests in which they dwell; and the female of the gorgeous fire-back pheasant, *Lophura viellottii*, is of a very similar rich brown colour.

These and many other colours of female birds seem to me exactly analogous to the colours of *both sexes* in such groups as the snipes, woodcocks, plovers, ptarmigan, desert birds, arctic animals, greenbirds.

[The second page of this letter has been torn off. This letter and that of Sept. 27 appear both to answer the same letter from Darwin. The last page of this or of another letter was placed with it in the portfolio of letters; it is now given.]

I am sorry to find that our difference of opinion on this point is a source of anxiety to you.

Pray do not let it be so. The truth will come out at last, and our difference may be the means of setting others to work who may set us both right.

After all, this question is only an episode (though an important one) in the great question of the origin of species, and whether you or I are right will not at all affect the main doctrine—that is one comfort.

I hope you will publish your treatise on Sexual Selection as a separate book as soon as possible, and then while you are going on with your other work, there will no doubt be found someone to battle with me over your facts, on this hard problem.

With best wishes and kind regards to Mrs. Darwin and all your family, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. October 6, 1868.

My dear Wallace,—Your letter is very valuable to me, and in every way very kind. I will not inflict a long answer, but only answer your queries. There are breeds (viz. Hamburgh) in which both sexes differ much from each other and from both sexes of *G. bankiva*; and both sexes are kept constant by selection.

The comb of Spanish ♂ has been ordered to be upright, and that of Spanish ♀ to lop over, and this has been effected. There are sub-breeds of game fowl, with ♀s very distinct and ♂s almost

identical; but this apparently is the result of spontaneous variation without special selection.

I am very glad to hear of the case of ♀ birds of paradise.

I have never in the least doubted the possibility of modifying female birds *alone* for protection; and I have long believed it for butterflies; I have wanted only evidence for the females alone of birds having had their colours modified for protection. But then I believe that the variations by which a female bird or butterfly could get or has got protective colouring have probably from the first been variations limited in their transmission to the female sex; and so with the variations of the male, where the male is more beautiful than the female, I believe the variations were sexually limited in their transmission to the males. I am delighted to hear that you have been hard at work on your MS.—Yours most sincerely,

CH. DARWIN.

9 St. Mark's Crescent, N.W. January 20, 1869.

Dear Darwin,—It will give me very great pleasure if you will allow me to dedicate my little book of Malayan Travels to you, although it will be far too small and unpretending a work to be worthy of that honour. Still, I have done what I can to make it a vehicle for communicating a taste for the higher branches of Natural History, and I know that you will judge it only too favourably. We are in the middle of the second volume, and if the printers will get on, shall be out next month.

Have you seen in the last number of the *Quarterly Journal of Science* the excellent remarks on *Fraser's* article on Natural Selection failing as to Man? In one page it gets to the heart of the question, and I have written to the Editor to ask who the author is.

My friend Spruce's paper on Palms is to be read to-morrow evening at the Linnean. He tells me it contains a discovery which he calls "alteration of function." He found a clump of *Geonema* all of which were females, and the next year the same clump were all males! He has found other facts analogous to this, and I have no doubt the subject is one that will interest you.

Hoping you are pretty well and are getting on steadily with

your next volumes, and with kind regards to Mrs. Darwin and all your circle, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

P.S.—Have you seen the admirable article in the *Guardian* (!) on Lyell's *Principles*? It is most excellent and liberal. It is written by the Rev. Geo. Buckle, of Tiverton Vicarage, Bath, whom I met at Norwich and found a thoroughly scientific and liberal parson. Perhaps you have heard that I have undertaken to write an article for the *Quarterly* (!) on the same subject, to make up for that on "Modern Geology" last year not mentioning Sir C. Lyell.

Really, what with the Tories passing Radical Reform Bills and the Church periodicals advocating Darwinianism, the millennium must be at hand.—A. R. W.

Down, Bromley, Kent, S.E. January 22, 1869.

My dear Wallace,—Your intended dedication pleases me much and I look at it as a *great* honour, and this is nothing more than the truth. I am glad to hear for Lyell's sake and on general grounds that you are going to write in the *Quarterly*. Some little time ago I was actually wishing that you wrote in the *Quarterly*, as I knew that you occasionally contributed to periodicals, and I thought that your articles would thus be more widely read.

Thank you for telling me about the *Guardian*, which I will borrow from Lyell. I did note the article in the *Quarterly Journal of Science* and put it aside to read again with the articles in *Fraser* and the *Spectator*.

I have been interrupted in my regular work in preparing a new edition¹ of the "Origin," which has cost me much labour, and which I hope I have considerably improved in two or three important points. I always thought individual differences more important than single variations, but now I have come to the conclusion that they are of paramount importance, and in this I believe I agree with you. Fleeming Jenkin's arguments have convinced me.²

I heartily congratulate you on your new book being so nearly finished.—Believe me, my dear Wallace, yours very sincerely,

CH. DARWIN.

¹ The fifth, ² Explained in letter of February 2, 1869. See p. 192.

9 *St. Mark's Crescent, N.W. January 30, 1869.*

Dear Darwin,—Will you tell me *where* are Fleeming Jenkin's arguments on the importance of single variation? Because I at present hold most strongly the contrary opinion, that it is the individual differences or *general variability* of species that enables them to become modified and adapted to new conditions.

Variations or "sports" may be important in modifying an animal in one direction, as in colour for instance, but how it can possibly work in changes requiring co-ordination of many parts, as in Orchids for example, I cannot conceive. And as all the more important structural modifications of animals and plants imply much co-ordination, it appears to me that the chances are millions to one against *individual variations* ever coinciding so as to render the required modification possible. However, let me read first what has convinced you.

You may tell Mrs. Darwin that I have now a daughter.

Give my kind regards to her and all your family.—Very truly
yours, ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. February 2, 1869.

My dear Wallace,—I must have expressed myself atrociously; I meant to say exactly the reverse of what you have understood. F. Jenkin argued in the *North American Review*¹ against single variations ever being perpetuated, and has convinced me, though not in quite so broad a manner as here put. I always thought individual differences more important, but I was blind and thought that single variations might be preserved much oftener than I now see is possible or probable. I mentioned this in my former note merely because I believed that you had come to similar conclusions, and I like much to be in accord with you. I believe I was mainly deceived by single variations offering such simple illustrations, as when man selects.

We heartily congratulate you on the birth of your little daughter.—Yours very sincerely,
C. DARWIN.

Down, Bromley, Kent, S.E. March 5, 1869.

My dear Wallace,—I was delighted at receiving your book² this morning. The whole appearance and the illustrations with

¹ June, 1867.

² "Malay Archipelago."

which it [is] so profusely ornamented are quite beautiful. Blessings on you and your publisher for having the pages cut and gilded.

As for the dedication, putting quite aside how far I deserve what you say, it seems to me decidedly the best expressed dedication which I have ever met.

The reading will probably last me a month, for I dare not have it read aloud, as I know that it will set me thinking.

I see that many points will interest me greatly. When I have finished, if I have anything particular to say, I will write again. Accept my cordial thanks. The dedication is a thing for my children's children to be proud of.—Yours most sincerely,

CH. DARWIN.

9 St. Mark's Crescent, N.W. March 10, 1869.

Dear Darwin,—Thanks for your kind note. I could not persuade Mr. Macmillan to cut more than twenty-five copies for my own friends, and he even seemed to think this a sign of most strange and barbarous taste.

Mr. Weir's paper on the kinds of larvæ, etc., eaten or rejected by insectivorous birds was read at the last meeting of the Entomological Society and was most interesting and satisfactory. His observations and experiments, so far as they have yet gone, confirm in *every instance* my hypothetical explanation of the colours of caterpillars. He finds that all nocturnal-feeding obscure-coloured caterpillars, all *green* and *brown* and *mimicking* caterpillars, are greedily eaten by almost every insectivorous bird. On the other hand, every gaily coloured, spotted, or banded species, which never conceal themselves, and all spiny and hairy kinds are *invariably rejected*, either without or after trial. He has also come to the curious and rather unexpected conclusion, that hairy and spiny caterpillars are not protected by their hairs, but by their nauseous taste, the hairs being merely an external mark of their uneatableness, like the gay colours of others. He deduces this from two kinds of facts: (1) that very young caterpillars before the hairs are developed are equally rejected, and (2) that in many cases the smooth pupæ and even the perfect insects of the same species are equally rejected.

His facts, it is true, are at present not very numerous, but they all point one way. They seem to me to lend an immense support to my view of the great importance of protection in determining colour, for it has not only prevented the eatable species from ever acquiring bright colours, spots, or markings injurious to them, but it has also conferred on all the nauseous species distinguishing marks to render their uneatableness more protective to them than it would otherwise be. When you have read my book I shall be glad of any hints for corrections if it comes to another edition. I was horrified myself by coming accidentally on several verbal inelegancies after all my trouble in correcting, and I have no doubt there are many more important errors.—Believe me, dear Darwin, yours very truly,

ALFRED R. WALLACE.

Down, Bromley, Kent, S.E. March 22, 1869.

My dear Wallace,—I have finished your book.¹ It seems to me excellent, and at the same time most pleasant to read. That you ever returned alive is wonderful after all your risks from illness and sea voyages, especially that most interesting one to Waigiou and back. Of all the impressions which I have received from your book, the strongest is that your perseverance in the cause of science was heroic. Your descriptions of catching the splendid butterflies have made me quite envious, and at the same time have made me feel almost young again, so vividly have they brought before my mind old days when I collected, though I never made such captures as yours. Certainly collecting is the best sport in the world. I shall be astonished if your book has not a great success; and your splendid generalisations on geographical distribution, with which I am familiar from your papers, will be new to most of your readers. I think I enjoyed most the Timor case, as it is best demonstrated; but perhaps Celebes is really the most valuable. I should prefer looking at the whole Asiatic continent as having formerly been more African in its fauna, than admitting the former existence of a continent across the Indian Ocean. DeCaisne's paper on the flora of Timor, in which he points out its close relation to that of the Mascarene Islands, supports your

¹ "Malay Archipelago."

view. On the other hand, I might advance the giraffes, etc., in the Sewalik deposits. How I wish someone would collect the plants of Banca! The puzzle of Java, Sumatra, and Borneo is like the three geese and foxes: I have a wish to extend Malacca through Banca to part of Java and thus make three parallel peninsulas, but I cannot get the geese and foxes across the river.

Many parts of your book have interested me much: I always wished to hear an independent judgment about the Rajah Brooke, and now I have been delighted with your splendid eulogium on him.

With respect to the fewness and inconspicuousness of the flowers in the tropics, may it not be accounted for by the hosts of insects, so that there is no need for the flowers to be conspicuous? As, according to Humboldt, fewer plants are social in the tropical than in the temperate regions, the flowers in the former would not make so great a show.

In your note you speak of observing some inelegancies of style. I notice none. All is as clear as daylight. I have detected two or three errata.

In Vol. I. you write *londiacus*: is this not an error?

Vol. II., p. 236: for *western* side of Aru read *eastern*.

Page 315: Do you not mean the horns of the moose? For the elk has not palmated horns.

I have only one criticism of a general nature, and I am not sure that other geologists would agree with me: you repeatedly speak as if the pouring out of lava, etc., from volcanoes actually caused the subsidence of an adjoining area. I quite agree that areas undergoing opposite movements are somehow connected; but volcanic outbursts must, I think, be looked at as mere accidents in the swelling up of a great dome or surface of *plutonic* rocks; and there seems no more reason to conclude that such swelling or elevation in mass is the cause of the subsidence than that the subsidence is the cause of the elevation; which latter view is indeed held by some geologists. I have regretted to find so little about the habits of the many animals which you have seen.

In Vol. II., p. 399, I wish I could see the connection between variations having been first or long ago selected, and their appearance at an earlier age in birds of paradise than the variations which have subsequently arisen and been selected. In fact,

I do not understand your explanation of the curious order of development of the ornaments of these birds.

Will you please to tell me whether you are sure that the female Casuarius (Vol. II., p. 150) sits on her eggs as well as the male?—for, if I am not mistaken, Bartlett told me that the male alone, who is less brightly coloured about the neck, sits on the eggs. In Vol. II., p. 255, you speak of male savages ornamenting themselves more than the women, of which I have heard before; now, have you any notion whether they do this to please themselves, or to excite the admiration of their fellow-men, or to please the women, or, as is perhaps probable, from all three motives?

Finally, let me congratulate you heartily on having written so excellent a book, full of thought on all sorts of subjects. Once again, let me thank you for the very great honour which you have done me by your dedication.—Believe me, my dear Wallace, yours very sincerely,

CH. DARWIN.

Vol. II., p. 455: When in New Zealand I thought the inhabitants a mixed race, with the type of Tahiti preponderating over some darker race with more frizzled hair; and now that the stone instruments revealed the existence of ancient inhabitants, is it not probable that these islands were inhabited by true Papuans? Judging from descriptions the pure Tahitans must differ much from your Papuans.

The reference in the following letter is to Wallace's review, in the April number of the *Quarterly*, of Lyell's "Principles of Geology" (tenth edition), and of the sixth edition of the "Elements of Geology." Wallace points out that here for the first time Sir C. Lyell gave up his opposition to Evolution; and this leads Wallace to give a short account of the views set forth in the "Origin of Species." In this article Wallace makes a definite statement as to his views on the evolution of man, which were opposed to those of Darwin. He upholds the view that the brain of man, as well as the organs of speech, the hand and the external form, could not have been evolved by Natural Selection (the "child" he is supposed to "murder"). At p. 391 he writes: "In the brain of the lowest savages and, as far as we

know, of the prehistoric races, we have an organ . . . little inferior in size and complexity to that of the highest types. . . . But the mental requirements of the lowest savages, such as the Australians or the Andaman Islanders, are very little above those of many animals. . . . How then was an organ developed far beyond the needs of its possessor? Natural Selection could only have endowed the savage with a brain a little superior to that of an ape, whereas he actually possesses one but very little inferior to that of the average members of our learned societies."

This passage is marked in Darwin's copy with a triply underlined "No," and with a shower of notes of exclamation. It was probably the first occasion on which he realised the extent of this great and striking divergence in opinion between himself and his colleague. He had, however, some indication of it in Wallace's paper on Man in the *Anthropological Review*, 1864, referred to in his letter to Wallace of May 28, 1864 and again in that of April 14, 1869.

Down, Bromley, Kent, S.E. March 27, 1869.

My dear Wallace,— I must send a line to thank you, but this note will require no answer. This very morning after writing I found that "elk" was used for "moose" in Sweden, but I had been reading lately about elk and moose in North America.

As you put the case in your letter, which I think differs somewhat from your book, I am inclined to agree, and had thought that a feather could hardly be increased in length until it had first grown to full length, and therefore it would be increased late in life and transmitted to a corresponding age. But the Crossoptilon pheasant, and even the common pheasant, show that the tail feathers can be developed very early.

Thanks for other facts, which I will reflect on when I go again over my MS.

I read all that you said about the Dutch Government with much interest, but I do not feel I know enough to form any opinion against yours.

I shall be intensely curious to read the *Quarterly*: I hope you have not murdered too completely your own and my child.

I have lately, i.e. in the new edition of the "Origin,"¹ been

¹ The fifth edition, pp. 150-7.

moderating my zeal, and attributing much more to mere useless variability. I did think I would send you the sheet, but I dare-say you would not care to see it, in which I discuss Nägeli's essay on Natural Selection not affecting characters of no functional importance, and which yet are of high classificatory importance.

Hooker is pretty well satisfied with what I have said on this head. It will be curious if we have hit on similar conclusions. You are about the last man in England who would deviate a hair's breadth from his conviction to please any editor in the world.—Yours very sincerely,

CH. DARWIN.

P.S.—After all, I have thought of one question, but if I receive no answer I shall understand that (as is probable) you have nothing to say. I have seen it remarked that the men and women of certain tribes differ a little in shade or tint; but have you ever seen or heard of any difference in tint between the two sexes which did not appear to follow from a difference in habits of life?

Down, Bromley, Kent, S.E. April 14, 1869.

My dear Wallace,—I have been wonderfully interested by your article,¹ and I should think Lyell will be much gratified by it. I declare if I had been editor and had the power of directing you I should have selected for discussion the very points which you have chosen. I have often said to younger geologists (for I began in the year 1830) that they did not know what a revolution Lyell had effected; nevertheless, your extracts from Cuvier² have quite astonished me.

Though not able really to judge, I am inclined to put more confidence in Croll³ than you seem to do; but I have been much struck by many of your remarks on degradation.

Thomson's⁴ views of the recent age of the world have been for some time one of my sorest troubles, and so I have been glad to read what you say. Your exposition of Natural Selec-

¹ In the *Quarterly Review*, April, 1869.

² Cuvier, the French comparative anatomist, born 1769, died 1832.

³ James Croll, Scottish physicist, born 1821, died 1890.

⁴ Sir William Thomson, P.R.S., Lord Kelvin, born 1824.

tion seems to me inimitably good; there never lived a better expounder than you.

I was also much pleased at your discussing the difference between our views and Lamarck's.¹ One sometimes sees the odious expression, "Justice to myself compels me to say, etc.," but you are the only man I ever heard of who persistently does himself an injustice and never demands justice. Indeed, you ought in the review to have alluded to your paper in the *Linnean Journal*, and I feel sure all our friends will agree in this, but you cannot "Burke" yourself, however much you may try, as may be seen in half the articles which appear.

I was asked but the other day by a German professor for your paper, which I sent him. Altogether, I look at your article as appearing in the *Quarterly* as an immense triumph for our cause. I presume that your remarks on Man are those to which you alluded in your note.

If you had not told me I should have thought that they had been added by someone else. As you expected, I differ grievously from you, and I am very sorry for it.

I can see no necessity for calling in an additional and proximate cause in regard to Man. But the subject is too long for a letter.

I have been particularly glad to read your discussion, because I am now writing and thinking much about Man.

I hope that your Malay book sells well. I was extremely pleased with the article in the *Q. J. of Science*, inasmuch as it is thoroughly appreciative of your work. Alas! you will probably agree with what the writer says about the uses of the bamboo.

I hear that there is also a good article in the *Saturday Review*, but have heard nothing more about it.—Believe me, my dear Wallace, yours ever sincerely,
CH. DARWIN.

P.S.—I have had a baddish fall, my horse partly rolling over me; but I am getting rapidly well.

9 St. Mark's Crescent, N.W. April 18, 1869.

Dear Darwin,—I am very glad you think I have done justice to Lyell, and have also well "exposed" (as a Frenchman would

¹ Jean Baptiste Lamarck, French naturalist and physicist, born 1744, died 1829.

say) Natural Selection. There is nothing I like better than writing a little account of it, and trying to make it clear to the meanest capacity.

The "Croll" question is awfully difficult. I had gone into it more fully, but the Editor made me cut out eight pages.

I am very sorry indeed to hear of your accident, but trust you will soon recover and that it will leave no bad effects.

I can quite comprehend your feelings with regard to my "unscientific" opinions as to Man, because a few years back I should myself have looked at them as equally wild and uncalled for. I shall look with extreme interest for what you are writing on Man, and shall give full weight to any explanations you can give of his probable origin. My opinions on the subject have been modified solely by the consideration of a series of remarkable phenomena, physical and mental, which I have now had every opportunity of fully testing, and which demonstrate the existence of forces and influences not yet recognised by science. This will, I know, seem to you like some mental hallucination, but as I can assure you from personal communication with them, that Robert Chambers,¹ Dr. Norris of Birmingham, the well-known physiologist, and C. F. Varley,² the well-known electrician, who have all investigated the subject for years, agree with me both as to the facts and as to the main inferences to be drawn from them, I am in hopes that you will suspend your judgment for a time till we exhibit some corroborative symptoms of insanity.

In the meantime I can console you by the assurance that I *don't* agree with the *Q. J. of Science* about bamboo, and that I see no cause to modify any of my opinions expressed in my article on the "Reign of Law."—Believe me yours very faithfully,

ALFRED R. WALLACE.

9 St. Mark's Crescent, N.W. June 23, 1869.

Dear Darwin,—Thank you very much for the copy of your fifth edition of the "Origin." I have not yet read all the additions, but those I have looked at seem very interesting, though

¹ Robert Chambers, LL.D., author of "Vestiges of Creation," etc. (1802–1871).

² Cromwell Fleetwood Varley, F.R.S. (1828–1883).

somewhat brief, but I suppose you are afraid of its great and rapid growth.

A difficult sexual character seems to me the plumules or battledore scales on the wings of certain families and genera of butterflies, almost invariably changing in form with the species and genera in proportion to other changes, and always constant in each species yet confined to the males, and so small and mixed up with the other scales as to produce no effect on the colour or marking of the wings. How could sexual selection produce them?

Your correspondent Mr. Geach is now in England, and if you would like to see him I am sure would be glad to meet you. He is staying with his brother (address Guildford), but often comes to town.

Hoping that you have quite recovered from your accident and that the *great work* is progressing, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

P.S.—You will perhaps be pleased to hear that German, French, and Danish translations of my “Malay Archipelago” are in progress.—A. R. W.

Cærdon, Barmouth, N. Wales. June 25, 1869.

My dear Wallace,—We have been here a fortnight, and shall remain here till the beginning of August. I can say nothing good about my health, and I am so weak that I can hardly crawl half a mile from the house; but I hope I may improve, and anyhow the magnificent view of Cader is enjoyable.

I do not know that I have anything to ask Mr. Geach, nor do I suppose I shall be in London till late in the autumn, but I should be particularly obliged, if you have any communication with Mr. Geach, if you would express for me my *sincere* thanks for his kindness in sending me the very valuable answers on Expression. I wrote some months ago to him in answer to his last letter.

I would ask him to Down, but the fatigue to me of receiving a stranger is something which to you would be utterly unintelligible.

I think I have heard of the scales on butterflies; but there are

lots of sexual characters which quite baffle all powers of even conjecture.

You are quite correct, that I felt forced to make all additions to the "Origin" as short as possible.

I am indeed pleased to hear, and fully expected, that your Malay work would be known throughout Europe.

Oh dear! what would I not give for a little more strength to get on with my work.—Ever yours,

C. DARWIN.

I wish that you could have told me that your place in the new Museum was all settled.

9 St. Mark's Crescent, N.W. October 20, 1869.

Dear Darwin,—I do not know your son's (Mr. George Darwin's) address at Cambridge. Will you be so good as to forward him the enclosed note begging for a little information.

I was delighted to see the notice in the *Academy* that you are really going to bring out your book on Man. I anticipate for it an enormous sale, and shall read it with intense interest, although I expect to find in it more to differ from than in any of your other books. Some reasonable and reasoning opponents are now taking the field. I have been writing a little notice of Murphy's "Habit and Intelligence," which, with much that is strange and unintelligible, contains some very acute criticisms and the statement of a few real difficulties. Another article just sent me from the *Month* contains some good criticism. How incipient organs can be useful is a real difficulty, so is the independent origin of similar complex organs; but most of his other points, though well put, are not very formidable. I am trying to begin a little book on the Distribution of Animals, but I fear I shall not make much of it from my idleness in collecting facts.

I shall make it a popular sketch first, and, if it succeeds, gather materials for enlarging it at a future time. If any suggestion occurs to you as to the kind of maps that would be best, or on any other essential point, I should be glad of a hint. I hope your residence in Wales did you good. I had no idea you were so near Dolgelly till I met your son there one evening when I was going to leave the next morning. It is a glorious country, but the time I like is May and June—the foliage is so glorious.

Sincerely hoping you are pretty well, and with kind regards to Mrs. Darwin and the rest of your family, believe me yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent, S.E. October 21, 1869.

My dear Wallace,—I forwarded your letter at once to my son George, but I am nearly sure that he will not be able to tell you anything; I wish he could for my own sake; but I suspect there are few men in England who could. Pray send me a copy or tell me where your article on Murphy will be published. I have just received the *Month*, but have only read half as yet. I wish I knew who was the author; you ought to know, as he admires you so much; he has a wonderful deal of knowledge, but his difficulties have not troubled me much as yet, except the case of the dipterous larva. My book will not be published for a long time, but Murray wished to insert some notice of it. Sexual selection has been a tremendous job. Fate has ordained that almost every point on which we differ should be crowded into this vol. Have you seen the October number of the *Revue des deux Mondes*? It has an article on you, but I have not yet read it; and another article, not yet read, by a very good man on the Transformist School.

I am very glad to hear that you are beginning a book, but do not let it be "little," on Distribution, etc. I have no hints to give about maps; the subject would require long and anxious consideration. Before Forbes published his essay on Distribution and the Glacial Period I wrote out and had *copied* an essay on the same subject, which Hooker read. If this MS. would be of any use to you, *on account of the references* in it to papers, etc., I should be very glad to lend it, to be used in any way; for I foresee that my strength will never last out to come to this subject.

I have been pretty well since my return from Wales, though at the time it did me no good.

We shall be in London next month, when I shall hope to see you.—My dear Wallace, yours very sincerely, CH. DARWIN.

9 St. Mark's Crescent, N.W. December 4 [1869].

Dear Darwin,—Dr. Adolf Bernhard Meyer, who translated my book into German, has written to me for permission to

translate my original paper in the *Linnean Proceedings* with yours, and wants to put my photograph and yours in it. If you have given him permission to translate the papers (which I suppose he can do without permission if he pleases) I write to ask which of your photographs you would wish to represent you in Germany—the last, or the previous one by Ernest Edwards, which I think much the best, as if you like I will undertake to order them and save you any more trouble about it. It is, of course, out of the question our meeting to be photographed together, as Mr. Meyer coolly proposes.

Hoping you are well, believe me yours very faithfully,

ALFRED R. WALLACE.

P.S.—I have written a paper on Geological Time, which will appear in *Nature*, and I *think* I have hit upon a solution of your greatest difficulties in that matter.—A. R. W.

Down, Beckenham, Kent, S.E. December 5, 1869.

My dear Wallace,—I wrote to Dr. Meyer that the photographs in England would cost much and that they did not seem to me worth the cost to him, but that I of course had no sort of objection. I should be greatly obliged if you would kindly take the trouble to order any one which you think best: possibly it would be best to wait, unless you feel sure, till you hear again from Dr. M. I sent him a copy of our joint paper. He has kindly sent me the translation of your book, which is splendidly got up, and which I thought I could not better use than by sending it to Fritz Müller in Brazil, who will appreciate it.

I liked your reviews on Mr. Murphy very much; they are capitally written, like everything which is turned out of your workshop. I was specially glad about the eye. If you agree with me, take some opportunity of bringing forward the case of perfected greyhound or racehorse, in proof of the possibility of the selection of many correlated variations. I have remarks on this head in my last book.

If you throw light on the want of geological time, may honour, eternal glory and blessings crowd thick on your head.—Yours most sincerely,

CH. DARWIN.

I forgot to say that I wrote to Dr. M. to say that I should not soon be in London, and that, of all things in the world, I hate most the bother of sitting for photographs, so I declined with many apologies. I have recently refused several applications.

9 *St. Mark's Crescent, N.W.* January 22, 1870.

Dear Darwin,—My paper on Geological Time having been in type nearly two months, and not knowing when it will appear, I have asked for a proof to send you, Huxley and Lyell. The latter part only contains what I think is new, and I shall be anxious to hear if it at all helps to get over your difficulties.

I have been lately revising and adding to my various papers bearing on the "Origin of Species," etc., and am going to print them in a volume immediately, under the title of "Contributions to the Theory of Natural Selection: A Series of Essays."

In the last, I put forth my heterodox opinions as to Man, and even venture to attack the Huxleyan philosophy!

Hoping you are quite well and are getting on with your Man book, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

P.S.—When you have read the proof and done with it, may I beg you to return it to me?—A. R. W.

Down, Beckenham, Kent, S. E. January 26, [1870.]

My dear Wallace,—I have been very much struck by your whole article (returned by this post), especially as to rate of denudation, for the still glaciated surfaces have of late most perplexed me. Also *especially* on the lesser mutations of climate during the last 60,000 years; for I quite think with you no cause so powerful in inducing specific changes through the consequent migrations. Your argument would be somewhat strengthened about organic changes having been formerly more rapid, if Sir W. Thomson is correct that physical changes were formerly more violent and abrupt.

The whole subject is so new and vast that I suppose you hardly expect anyone to be at once convinced, but that he should keep your view before his mind and let it ferment. This, I think, everyone will be forced to do. I have not as yet been

able to digest the fundamental notion of the shortened age of the sun and earth. Your whole paper seems to me admirably clear and well put. I may remark that Rüttimeyer has shown that several wild mammals in Switzerland since the neolithic period have had their dentition and, I *think*, general size *slightly* modified. I cannot believe that the Isthmus of Panama has been open since the commencement of the glacial period; for, notwithstanding the Fishes, so few shells, crustaceans, and, according to Agassiz, not one echinoderm is common to the sides. I am very glad you are going to publish all your papers on Natural Selection: I am sure you are right, and that they will do our cause much good.

But I groan over Man—you write like a metamorphosed (in retrograde direction) naturalist, and you the author of the best paper that ever appeared in the *Anthropological Review*! Eheu! Eheu! Eheu!—Your miserable friend,

C. DARWIN.

Down, Beckenham, Kent. March 31, 1870.

My dear Wallace,—Many thanks for the woodcut, which, judging from the rate at which I crawl on, will hardly be wanted till this time next year. Whether I shall have it reduced, or beg Mr. Macmillan for a stereotype, as you said I might, I have not yet decided.

I heartily congratulate you on your removal being over, and I much more heartily condole with myself at your having left London, for I shall thus miss my talks with you which I always greatly enjoy.

I was excessively pleased at your review of Galton, and I agree to every word of it. I must add that I have just re-read your article in the *Anthropological Review*, and I *defy* you to upset your own doctrine.—Ever yours very sincerely,

CH. DARWIN.

Down, Beckenham, Kent. April 20, [1870].

My dear Wallace,—I have just received your book ["Natural Selection"]¹ and read the preface. There never has been passed on me, or indeed on anyone, a higher eulogium than yours. I wish that I fully deserved it. Your modesty and candour are

¹ Inserted by A. R. W.

very far from new to me. I hope it is a satisfaction to you to reflect—and very few things in my life have been more satisfactory to me—that we have never felt any jealousy towards each other, though in one sense rivals. I believe that I can say this of myself with truth, and I am absolutely sure that it is true of you.

You have been a good Christian to give a list of your additions, for I want much to read them, and I should hardly have had time just at present to have gone through all your articles.

Of course, I shall immediately read those that are new or greatly altered, and I will endeavour to be as honest as can reasonably be expected. Your book looks remarkably well got up.—Believe me, my dear Wallace, to remain yours very cordially,

CH. DARWIN.

Down, Beckenham, Kent, S.E. June 5, 1870.

My dear Wallace,—As imitation and protection are your subjects I have thought that you would like to possess the enclosed curious drawing. The note tells all I know about it.—Yours very sincerely,

CH. DARWIN.

P.S.—I read not long ago a German article on the colours of *female* birds, and that author leaned rather strongly to your side about nidification. I forget who the author was, but he seemed to know a good deal.—C. D.

Holly House, Barking, E. July 6, 1870.

Dear Darwin,—Many thanks for the drawing. I must say, however, the resemblance to a snake is not very striking, unless to a cobra not found in America. It is also evident that it is not Mr. Bates's caterpillar, as that threw the head backwards so as to show the feet above, forming imitations of keeled scales.

Claparède has sent me his critique on my book. You will probably have it too. His arguments in reply to my heresy seem to me of the weakest. I hear you have gone to press, and I look forward with fear and trembling to being crushed under a mountain of facts!

I hear you were in town the other day. When you are again,

I should be glad to come at any convenient hour and give you a call.

Hoping your health is improving, and with kind remembrances to Mrs. Darwin and all your family, believe me, yours very faithfully,

ALFRED R. WALLACE.

In "My Life" (Vol. II., p. 7) Wallace wrote: "In the year 1870 Mr. A. W. Bennett read a paper before Section D of the British Association at Liverpool entitled 'The Theory of Natural Selection from a Mathematical Point of View,' and this paper was printed in full in *Nature* of November 10, 1870. To this I replied on November 17, and my reply so pleased Mr. Darwin that he at once wrote to me as follows":

Down, Beckenham, Kent, S.E. November 22, 1870.

My dear Wallace,—I must ease myself by writing a few words to say how much I and all others in this house admire your article in *Nature*. You are certainly an unparalleled master in lucidly stating a case and in arguing. Nothing ever was better done than your argument about the term "origin of species," and the consequences about much being gained, even if we know nothing about precise cause of each variation. By chance I have given a few words in my first volume, now some time printed off, about mimetic butterflies, and have touched on two of your points, viz. on species already widely dissimilar not being made to resemble each other, and about the variations in Lepidoptera being often well pronounced. How strange it is that Mr. Bennett or anyone else should bring in the action of the mind as a leading cause of variation, seeing the beautiful and complex adaptations and modifications of structure in plants, which I do not suppose they would say had minds.

I have finished the first volume, and am half-way through the first proof of the second volume, of my confounded book, which half kills me by 'fatigue, and which I much fear will quite kill me in your good estimation.

If you have leisure I should much like a little news of you and your doings and your family.—Ever yours very sincerely,

CH. DRAWIN.

Holly House, Barking, E. November 24, 1870.

Dear Darwin,—Your letter gave me very great pleasure. We still agree, I am sure, on nineteen points out of twenty, and on the twentieth I am not invincible. But then I must be convinced by facts and arguments, not by high-handed ridicule such as Claparède's.

I hope you see the difference between such criticisms as his, and that in the last number of the *North American Review*, where my last chapter is really criticised, point by point; and though I think some of it very weak, I admit that some is very strong, and almost converts me from the error of my ways.

As to your new book, I am sure it will not make me think less highly of you than I do, unless you do, what you have never done yet, ignore facts and arguments that go against you.

I am doing nothing just now but writing articles and putting down anti-Darwinians, being dreadfully ridden upon by a horrid old-man-of-the-sea, who has agreed to let me have the piece of land I have set my heart on, and which I have been trying to get of him since last February, but who will not answer letters, will not sign an agreement, and keeps me week after week in anxiety, though I have accepted his own terms unconditionally, one of which is that I pay rent from last Michaelmas! And now the finest weather for planting is going by. It is a bit of a wilderness that can be made into a splendid imitation of a Welsh valley in little, and will enable me to gather round me all the beauties of the temperate flora which I so much admire, or I would not put up with the little fellow's ways. The fixing on a residence for the rest of your life is an important event, and I am not likely to be in a very settled frame of mind for some time.

I am answering A. Murray's Geographical Distribution of Coleoptera for my Entomological Society Presidential Address, and am printing a second edition of my "Essays," with a few notes and additions. Very glad to see (by your writing yourself) that you are better, and with kind regards to all your family, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

Holly House, Barking, E. January 27, 1871.

Dear Darwin,—Many thanks for your first volume,¹ which I have just finished reading through with the greatest pleasure and interest, and I have also to thank you for the great tenderness with which you have treated me and my heresies.

On the subject of sexual selection and protection you do not yet convince me that I am wrong, but I expect your heaviest artillery will be brought up in your second volume, and I may have to capitulate. You seem, however, to have somewhat misunderstood my exact meaning, and I do not think the difference between us is quite so great as you seem to think it. There are a number of passages in which you argue against the view that the female has, in any large number of cases, been “specially modified” for protection, or that *colour* has *generally* been obtained by either sex for purposes of protection.

But my view is, and I thought I had made it clear, that the female has (in most cases) been simply prevented from acquiring the gay tints of the male (even when there was a tendency for her to inherit it) because it was hurtful; and, that when protection is not needed, gay colours are so generally acquired by both sexes as to show that inheritance by both sexes of colour variations is the most usual, when *not prevented from acting* by Natural Selection.

The colour itself may be acquired either by sexual selection or by other unknown causes. There are, however, difficulties in the very wide application you give to sexual selection which at present stagger me, though no one was or is more ready than myself to admit the perfect truth of the principle or the immense importance and great variety of its applications. Your chapters on Man are of intense interest, but as touching my special heresy not as yet altogether convincing, though of course I fully agree with every word and every argument which goes to prove the “evolution” or “development” of man out of a lower form. My only difficulties are as to whether you have accounted for *every* step of the development by ascertained laws. Feeling sure that the book will keep up and increase your high reputation and be immensely successful, as it deserves to be, believe me, dear Darwin, yours very faithfully, ALFRED R. WALLACE.

¹ “The Descent of Man.”

Down, Beckenham, Kent, S.E. January 30, 1871.

My dear Wallace,—Your note has given me very great pleasure, chiefly because I was so anxious not to treat you with the least disrespect, and it is so difficult to speak fairly when differing from anyone. If I had offended you, it would have grieved me more than you will readily believe. Secondly, I am greatly pleased to hear that Vol. I. interests you; I have got so sick of the whole subject that I felt in utter doubt about the value of any part. I intended when speaking of the female not having been specially modified for protection to include the prevention of characters acquired by the ♂ being transmitted to the ♀; but I now see it would have been better to have said "specially acted on," or some such term. Possibly my intention may be clearer in Vol. II. Let me say that my conclusions are chiefly founded on a consideration of all animals taken in a body, bearing in mind how common the rules of sexual differences appear to be in all classes. The first copy of the chapter on Lepidoptera agreed pretty closely with you. I then worked on, came back to Lepidoptera, and thought myself compelled to alter it, finished sexual selection, and for the last time went over Lepidoptera, and again I felt forced to alter it.

I hope to God there will be nothing disagreeable to you in Vol. II., and that I have spoken fairly of your views. I feel the more fearful on this head, because I have just read (but not with sufficient care) Mivart's book,¹ and I feel *absolutely certain* that he meant to be fair (but he was stimulated by theological fervour); yet I do not think he has been quite fair: he gives in one place only half of one of my sentences, ignores in many places all that I have said on effects of use, speaks of my dogmatic assertion, "of false belief," whereas the end of paragraph seems to me to render the sentence by no means dogmatic or arrogant; etc., etc. I have since its publication received some quite charming letters from him.

What an ardent (and most justly) admirer he is of you. His work, I do not doubt, will have a most potent influence versus Natural Selection. The pendulum will now swing against us. The part which, I think, will have most influence is when he gives whole series of cases, like that of whalebone, in which

¹ "The Genesis of Species," by St. G. Mivart, 1871.

we cannot explain the gradational steps; but such cases have no weight on my mind—if a few fish were extinct, who on earth would have ventured even to conjecture that lung had originated in swim-bladder? In such a case as Thylacines, I think he was bound to say that the resemblance of the jaw to that of the dog is superficial; the number and correspondence and development of teeth being widely different. I think, again, when speaking of the necessity of altering a number of characters together, he ought to have thought of man having power by selection to modify simultaneously or almost simultaneously many points, as in making a greyhound or racehorse — as enlarged upon in my “Domestic Animals.”

Mivart is savage or contemptuous about my “moral sense,” and so probably will you be. I am extremely pleased that he agrees with my position, *as far as animal nature is concerned*, of man in the series; or, if anything, thinks I have erred in making him too distinct.

Forgive me for scribbling at such length.

You have put me quite in good spirits, I did so dread having been unintentionally unfair towards your views. I hope earnestly the second volume will escape as well. I care now very little what others say. As for our not quite agreeing, really in such complex subjects it is almost impossible for two men who arrive independently at their conclusions to agree fully—it would be unnatural for them to do so.—Yours ever very sincerely,

CH. DARWIN.

Holly House, Barking, E. March 11, 1871.

Dear Darwin,—I need not say that I read your second volume with, if possible, a greater interest than the first, as so many topics of special interest to me are treated of. You will not be surprised to find that you have not convinced me on the “female protection” question, but you *will* be surprised to hear that I do not despair of convincing you. I have been writing, as you are aware, a review for the *Academy*, which I tried to refuse doing, but the Editor used as an argument the statement that you wished me to do so. It is not an easy job fairly to summarise such a book, but I hope I have succeeded tolerably. When I got to discussion, I felt more at home, but I most

sincerely trust that I may not have let pass any word that may seem to you in the least too strong.

You have not written a word about me that I could wish altered, but as I know you wish me to be candid with you, I will mention that you have quoted one passage in a note (p. 376, Vol. II.) which seems to me a caricature of anything I have written.

Now let me ask you to rejoice with me, for I have got my chalk pit, and am hard at work engineering a road up its precipitous slopes. I hope you may be able to come and see me there some day, as it is an easy ride from London, and I shall be anxious to know if it is equal to the pit in the wilds of Kent Mrs. Darwin mentioned when I lunched with you. Should your gardener in the autumn have any thinnings out of almost any kind of hardy plants they would be welcome, as I have near four acres of ground in which I want to substitute ornamental plants for weeds.

With best wishes, and hoping you may have health and strength to go on with your great work, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

My review will appear next Wednesday.

Down, Beckenham, Kent, S.E. March 16, 1871.

My dear Wallace,—I have just read your grand review.¹ It is in every way as kindly expressed towards myself as it is excellent in matter. The Lyells have been here, and Sir C. remarked that no one wrote such good scientific reviews as you, and, as Miss Buckley added, you delight in picking out all that is good, though very far from blind to the bad. In all this I most entirely agree. I shall always consider your review as a great honour, and however much my book may hereafter be abused, as no doubt it will be, your review will console me, notwithstanding that we differ so greatly.

I will keep your objections to my views in my mind, but I fear that the latter are almost stereotyped in my mind. I

¹ In the *Academy*, March 15, 1871.

thought for long weeks about the inheritance and selection difficulty, and covered quires of paper with notes, in trying to get out of it, but could not, though clearly seeing that it would be a great relief if I could. I will confine myself to two or three remarks. I have been much impressed with what you urge against colour¹ in the case of insects having been acquired through sexual selection. I always saw that the evidence was very weak; but I still think, if it be admitted that the musical instruments of insects have been gained through sexual selection, that there is not the least improbability in colour having been thus gained. Your argument with respect to the denudation of mankind, and also to insects, that taste on the part of one sex would have to remain nearly the same during many generations, in order that sexual selection should produce any effect, I agree to, and I think this argument would be sound if used by one who denied that, for instance, the plumes of birds of paradise had been so gained.

I believe that you admit this, and if so I do not see how your argument applies in other cases. I have recognised for some short time that I have made a great omission in not having discussed, as far as I could, the acquisition of taste, its inherited nature, and its permanence within pretty close limits for long periods.

One other point and I have done: I see by p. 179 of your review that I must have expressed myself very badly to have led you to think that I consider the prehensile organs of males as affording evidence of the females exerting a choice. I have never thought so, and if you chance to remember the passage (but do not hunt for it), pray point it out to me.

I am extremely sorry that I gave the note from Mr. Stebbing; I thought myself bound to notice his suggestion of beauty as a cause of denudation, and thus I was led on to give his argument. I altered the final passage which seemed to me offensive, and I had misgivings about the first part.

¹ "Mr. Wallace says that the pairing of butterflies is probably determined by the fact that one male is stronger-winged or more pertinacious than the rest, rather than by the choice of the females. He quotes the case of caterpillars which are brightly coloured and yet sexless. Mr. Wallace also makes the good criticism that 'The Descent of Man' consists of two books mixed together."—"Life and Letters of Charles Darwin," iii. 137.

I heartily wish I had yielded to these misgivings. I will omit in any future edition the latter half of the note.

I have heard from Miss Buckley that you have got possession of your chalk pit, and I congratulate you on the tedious delay being over. I fear all our bushes are so large that there is nothing which we are at all likely to grub up.

Years ago we threw away loads of things. I should very much like to see your house and grounds; but I fear the journey would be too long. Going even to Kew knocks me up, and I have almost ceased trying to do so.

Once again let me thank you warmly for your admirable review.—My dear Wallace, yours ever very sincerely,

C. DARWIN.

What an excellent address you gave about Madeira, but I wish you had alluded to Lyell's discussion on land-shells, etc., not that he has said a word on the subject. The whole address quite delighted me. I hear Mr. Crotch¹ disputed some of your facts about the wingless insects, but he is a *crotchety* man. As far as I remember, I did not venture to ask Mr. Appleton to get you to review me, but only said, in answer to an inquiry, that you would undoubtedly be the best, or one of the very few men who could do so effectively.

Down, Beckenham, Kent, S.E. March 24, 1871.

My dear Wallace,—Very many thanks for the new edition of your Essays. Honour and glory to you for giving list of additions. It is grand as showing that our subject flourishes, your book coming to a new edition so soon. My book also sells immensely; the edition will, I believe, be 6,500 copies. I am tired with writing, for the load of letters which I receive is enough to make a man cry, yet some few are curious and valuable. I got one to-day from a doctor on the hair on backs of young weakly children, which afterwards falls off. Also on hairy idiots. But I am tired to death, so farewell.

Thanks for your last letter.

There is a very striking second article on my book in the

¹ G. Crotch is a well-known coleopterist and official in the University Library at Cambridge.

Pall Mall. The articles in the *Spectator*¹ have also interested me much.—Again farewell.

C. DARWIN.

Holly House, Barking, E. May 14, 1871.

Dear Darwin,—Have you read that very remarkable book “The Fuel of the Sun”? If not, get it. It solves the great problem of the almost unlimited duration of the sun’s heat in what appears to me a most satisfactory manner. I recommended it to Sir C. Lyell, and he tells me that Grove spoke very highly of it to him. It has been somewhat ignored by the critics because it is by a new man with a perfectly original hypothesis, founded on a vast accumulation of physical and chemical facts; but not being encumbered with any mathematical shibboleths, they have evidently been afraid that anything so intelligible could not be sound. The manner in which everything in physical astronomy is explained is almost as marvellous as the powers of Natural Selection in the same way, and naturally excites a suspicion that the respective authors are pushing their theories “a little too far.”

If you read it, get Proctor’s book on the Sun at the same time, and refer to his coloured plates of the protuberances, corona, etc., which marvellously correspond with what Matthieu Williams’s theory requires. The author is a practical chemist engaged in iron manufacture, and it is from furnace chemistry that he has been led to the subject. I think it the most original, most thoughtful and most carefully-worked-out theory that has appeared for a long time, and it does not say much for the critics that, as far as I know, its great merits have not been properly recognised.

I have been so fully occupied with road-making, well-digging, garden- and house-planning, planting, etc., that I have given up all other work.

Do you not admire our friend Miss Buckley’s admirable

¹ *Spectator*, March 11 and 18, 1871. “With regard to the evolution of conscience the reviewer thinks that Mr. Darwin comes much nearer to the ‘kernel of the psychological problem’ than many of his predecessors. The second article contains a good discussion of the bearing of the book on the question of design, and concludes by finding in it a vindication of Theism more wonderful than that in Paley’s ‘Natural Theology.’”—“Life and Letters,” iii. 138.

article in *Macmillan*? It seems to me the best and most original that has been written on your book.

Hoping you are well, and are not working too hard, I remain
yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. July 9, 1871.

My dear Wallace,—I send by this post a review by Chauncey Wright, as I much want your opinion of it, as soon as you can send it. I consider you an incomparably better critic than I am. The article, though not very clearly written, and poor in parts for want of knowledge, seems to me admirable.

Mivart's book is producing a great effect against Natural Selection, and more especially against me. Therefore, if you think the article even somewhat good, I will write and get permission to publish it as a shilling pamphlet, together with the MS. addition (enclosed), for which there was not room at the end of the review. I do not suppose I should lose more than £20 or £30.

I am now at work at a new and cheap edition of the "Origin," and shall answer several points in Mivart's book and introduce a new chapter for this purpose; but I treat the subject so much more concretely, and I daresay less philosophically, than Wright, that we shall not interfere with each other. You will think me a bigot when I say, after studying Mivart, I was never before in my life so convinced of the *general* (i.e. not in detail) truth of the views in the "Origin." I grieve to see the omission of the words by Mivart, detected by Wright.¹ I complained to M. that in two cases he quotes only the commencement of sentences by me and thus modifies my meaning; but I never supposed he would have omitted words. There are other cases of what I consider unfair treatment. I conclude with sorrow that though he means to be honourable, he is so bigoted that he cannot act fairly.

I was glad to see your letter in *Nature*, though I think you were a little hard on the silly and presumptuous man.

¹ *North American Review*, Vol. 113, pp. 83, 84. Chauncey Wright points out that the words omitted are "essential to the point on which he [Mr. Mivart] cites Mr. Darwin's authority." It should be mentioned that the passage from which words are omitted is not given within inverted commas by Mr. Mivart.—See "Life and Letters of Charles Darwin," iii. 144.

I hope that your house and grounds are progressing well, and that you are in all ways flourishing.

I have been rather seedy, but a few days in London did me much good; and my dear good wife is going to take me somewhere, *nolens volens*, at the end of this month.

C. DARWIN.

Holly House, Barking, E. July 12, 1871.

Dear Darwin,—Many thanks for giving me the opportunity to read at my leisure the very talented article of Mr. C. Wright. His criticism of Mivart, though very severe, is, I think, in most cases sound; but I find the larger part of the article so heavy and much of the language and argument so very obscure, that I very much doubt the utility of printing it separately. I do not think the readers of Mivart could ever read it in that form, and I am sure your own answer to Mivart's arguments will be so much more clear and to the point that the other will be unnecessary. You might extract certain portions in your own chapter, such as the very ingenious suggestion as to the possible origin of mammary glands, as well as the possible use of the rattle of the rattlesnake, etc.

I cannot see the force of Mivart's objection to the theory of production of the long neck of the giraffe (suggested in my first Essay), and which C. Wright seems to admit, while his "watch-tower" theory seems to me more difficult and unlikely as a means of origin. The argument, "Why haven't other allied animals been modified in the same way?" seems to me the weakest of the weak. I must say also I do not see any great reason to complain of the "words" left out by Mivart, as they do not seem to me materially to affect the meaning. Your expression, "and tends to depart in a slight degree," I think hardly grammatical; a *tendency* to depart cannot very well be said to be in a slight degree; a *departure* can, but a tendency must be either a *slight tendency* or a *strong tendency*; the degree to which the departure may reach must depend on favourable or unfavourable causes in addition to the tendency itself. Mivart's words, "and tending to depart from the parental type," seem to me quite unobjectionable as a paraphrase of yours, because the "tending" is kept in; and your own view undoubtedly is that the tendency may lead to an ultimate departure

to any extent. Mivart's error is to suppose that your words favour the view of *sudden departures*, and I do not see that the expression he uses really favours his view a bit more than if he had quoted your exact words. The expression of yours he relies upon is evidently "the whole organism seeming to have become plastic," and he argues, no doubt erroneously, that having so become "plastic," any amount or a larger amount of sudden variation in some direction is likely.

Mivart's greatest error, the confounding "individual variations" with "minute or imperceptible variations," is well exposed by C. Wright, and that part I should like to see reprinted; but I always thought you laid too much stress on the slowness of the action of Natural Selection owing to the smallness and rarity of favourable variations. In your chapter on Natural Selection the expressions, "extremely slight modifications," "every variation even the slightest," "every grade of constitutional difference," occur, and these have led to errors such as Mivart's. I say all this because I feel sure that Mivart would be the last to intentionally misrepresent you, and he has told me that he was sorry the word "infinitesimal," as applied to variations used by Natural Selection, got into his book, and that he would alter it, as no doubt he has done, in his second edition.

Some of Mivart's strongest points—the eye and ear, for instance—are unnoticed in the review. You will, of course, reply to these. His statement of the "missing link" argument is also forcible, and has, I have no doubt, much weight with the public. As to all his minor arguments, I feel with you that they leave Natural Selection stronger than ever, while the two or three main arguments do leave a lingering doubt in my mind of some fundamental organic law of development of which we have as yet no notion.

Pray do not attach any weight to my opinions as to the review. It is very clever, but the writer seems a little like those critics who know an author's or an artist's meaning better than they do themselves.

My house is now in the hands of a contractor, but I am wall-building, etc., and very busy.—With best wishes, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. July 12, 1871.

My dear Wallace,—Very many thanks. As soon as I read your letter I determined not to print the paper, notwithstanding my eldest daughter, who is a very good critic, thought it so interesting as to be worth reprinting. Then my wife came in, and said, "I do not much care about these things and shall therefore be a good judge whether it is very dull." So I will leave my decision open for a day or two. Your letter has been, and will be, of use to me in other ways: thus I had quite forgotten that you had taken up the case of the giraffe in your first memoir, and I must look to this. I feel very doubtful how far I shall succeed in answering Mivart; it is so difficult to answer objections to doubtful points and make the discussion readable. I shall make only a selection. The worst of it is that I cannot possibly hunt through all my references for isolated points; it would take me three weeks of intolerably hard work. I wish I had your power of arguing clearly. At present I feel sick of everything, and if I could occupy my time and forget my daily discomforts or little miseries, I would never publish another word. But I shall cheer up, I daresay, soon, being only just got over a bad attack. Farewell. God knows why I bother you about myself.

I can say nothing more about missing links than what I have said. I should rely much on pre-Silurian times; but then comes Sir W. Thomson like an odious spectre. Farewell.—Yours most sincerely,

CH. DARWIN.

I was grieved to see in the *Daily News* that the madman about the flat earth has been threatening your life. What an odious trouble this must have been to you.

P.S.—There is a most cutting review of me in the *Quarterly*.¹ I have only read a few pages. The skill and style make me think of Mivart. I shall soon be viewed as the most despicable of men. This *Quarterly* review tempts me to republish Ch. Wright, even if not read by anyone, just to show that someone will say a word against Mivart, and that his (i.e. Mivart's) remarks ought not to be swallowed without some reflection.

¹ July, 1871.

I quite agree with what you say that Mivart fully intends to be honourable; but he seems to me to have the mind of a most able lawyer retained to plead against us, and especially against me. God knows whether my strength and spirit will last out to write a chapter versus Mivart and others; I do so hate controversy, and feel I should do it so badly.

P.S.—I have now finished the review: there can be no doubt it is by Mivart, and wonderfully clever.

Holly House, Barking, E. July 16, 1871.

Dear Darwin,—I am very sorry you are so unwell, and that you allow criticisms to worry you so. Remember the noble army of converts you have made! and the host of the most talented men living who support you wholly. What do you think of putting C. Wright's article as an appendix to the new edition of the "Origin"? That would get it read, and obviate my chief objection, that the people who read Mivart and the "Origin" will very few of them buy a separate pamphlet to read. Pamphlets are such nuisances. I don't think Mivart could have written the *Quarterly* article, but I will look at it and shall, I think, be able to tell. Pray keep your spirits up. I am so distracted by building troubles that I can write nothing, and I shall not, till I get settled in my new house, some time next spring, I hope.—With best wishes, believe me yours very faithfully,

ALFRED R. WALLACE.

Haredene, Albury, Guildford. August 1, 1871.

My dear Wallace,—Your kind and sympathetic letter pleased me greatly and did me good, but as you are so busy I did not answer it. I write now because I have just received a very remarkable letter from Fritz Müller (with butterflies' wings gummed on paper as illustrations) on mimicry, etc. I think it is well worth your reading, but I will not send it, unless I receive a ½d. card to this effect. He puts the difficulty of first start in imitation excellently, and gives wonderful proof of closeness of the imitation. He hints a curious addition to the theory in relation to sexual selection, which you will think madly hypothetical: it occurred to me in a very different class of cases,

but I was afraid to publish it. It would aid the theory of imitative protection, *when the colours are bright*. He seems much pleased with your caterpillar theory. I wish the letter could be published, but without coloured illustrations [it] would, I fear, be unintelligible.

I have not yet made up my mind about Wright's review; I shall stop till I hear from him. Your suggestion would make the "Origin," already too large, still more bulky.

By the way, did Mr. Youmans, of the United States, apply to you to write a popular sketch of Natural Selection? I told him you would do it immeasurably better than anyone in the world. My head keeps very rocky and wretched, but I am better.—Ever yours most truly,

C. DARWIN.

Hollyhouse, Barking, E. March 3, 1872.

Dear Darwin,—Many thanks for your new edition of the "Origin," which I have been too busy to acknowledge before. I think your answer to Mivart on the initial stages of modification ample and complete, and the comparison of whale and duck most beautiful. I always saw the fallacy of these objections, of course. The eye and ear objection you have not so satisfactorily answered, and to me the difficulty exists of how *three times over* an organ of sight was developed with the apparatus even approximately identical. Why should not, in one case out of the three, the heat rays or the chemical rays have been utilised for the same purpose, in which case no translucent media would have been required, and yet vision might have been just as perfect? The fact that the eyes of insects and molluscs are transparent to us shows that the very same limited portion of the rays of the spectrum is utilised for vision by them as by us.

The chances seem to me immense against that having occurred through "fortuitous variation," as Mivart puts it.

I see still further difficulties on this point but cannot go into them now. Many thanks for your kind invitation. I will try and call some day, but I am now very busy trying to make my house habitable by Lady Day, when I *must* be in it.—Believe me yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. July 27, 1872.

My dear Wallace,—I have just read with infinite satisfaction your crushing article in *Nature*.¹ I have been the more glad to see it, as I have not seen the book itself: I did not order it, as I felt sure from Dr. B.'s former book that he could write nothing of value. But assuredly I did not suppose that anyone would have written such a mass of inaccuracies and rubbish. How rich is everything which he says and quotes from Herbert Spencer!

By the way, I suppose that you read H. Spencer's answer to Martineau: it struck me as quite wonderfully good, and I felt even more strongly inclined than before to bow in reverence before him. Nothing has amused me more in your review than Dr. B.'s extraordinary presumption in deciding that such men as Lyell, Owen, H. Spencer, Mivart, Gaudry, etc., etc., are all wrong. I daresay it would be very delightful to feel such overwhelming confidence in oneself.

I have had a poor time of it of late, rarely having an hour of comfort, except when asleep or immersed in work; and then when that is over I feel dead with fatigue. I am now correcting my little book on Expression; but it will not be published till November, when of course a copy will be sent to you. I shall now try whether I can occupy myself without writing anything more on so difficult a subject as Evolution.

I hope you are now comfortably settled in your new house, and have more leisure than you have had for some time. I have looked out in the papers for any notice about the curatorship of the new Museum, but have seen nothing. If anything is decided in your favour, I beg you to inform me.—My dear Wallace, very truly yours,

C. DARWIN.

How grandly the public has taken up Hooker's case.

Down. August 3, [1872].

My dear Wallace,—I hate controversy, chiefly perhaps because I do it badly; but as Dr. Bree accuses you of "blundering," I have thought myself bound to send the enclosed letter² to

¹ A review of Dr. Bree's book, "An Exposition of Fallacies in the Hypotheses of Mr. Darwin."—*Nature*, July 25, 1872.

² "Bree on Darwinism," *Nature*, Aug. 8, 1872. The letter is as follows: "Permit me to state—though the statement is almost superfluous—that

Nature, that is, if you in the least desire it. In this case please post it. If you do not *at all* wish it, I should rather prefer not sending it, and in this case please tear it up. And I beg you to do the same, if you intend answering Dr. Bree yourself, as you will do it incomparably better than I should. Also please tear it up if you don't like the letter.—My dear Wallace, yours very sincerely,

CH. DARWIN.

The Dell, Grays, Essex. August 4, 1872.

Dear Darwin,—I have sent your letter to *Nature*, as I think it will settle that question far better than anything I can say. Many thanks for it. I have not seen Dr. Bree's letter yet, as I get *Nature* here very irregularly, but as I was very careful to mention none but *real errors* in Dr. Bree's book, I do not imagine there will be any necessity for my taking any notice of it. It was really entertaining to have such a book to review, the errors and misconceptions were so inexplicable and the self-sufficiency of the man so amazing. Yet there is some excellent writing in the book, and to a half-informed person it has all the appearance of being a most valuable and authoritative work.

I am now reviewing a much more important book and one that, if I mistake not, will really compel you sooner or later to modify some of your views, though it will not at all affect the main doctrine of Natural Selection as applied to the higher animals. I allude, of course, to Bastian's "Beginnings of Life," which you have no doubt got. It is hard reading, but intensely interesting. I am a thorough convert to his main results, and it seems to me that nothing more important has appeared since your "Origin." It is a pity he is so awfully voluminous and discursive. When you have thoroughly digested it I shall be glad

Mr. Wallace, in his review of Dr. Bree's work, gives with perfect correctness what I intended to express, and what I believe was expressed clearly, with respect to the probable position of man in the early part of his pedigree. As I have not seen Dr. Bree's recent work, and as his letter is unintelligible to me, I cannot even conjecture how he has so completely mistaken my meaning; but, perhaps, no one who has read Mr. Wallace's article, or who has read a work formerly published by Dr. Bree on the same subject as his recent one, will be surprised at any amount of misunderstanding on his part."—CHARLES DARWIN, Aug. 3. See "Life and Letters of Charles Darwin," iii. 167.

to know what you are disposed to think. My first notice of it will I think appear in *Nature* next week, but I have been hurried for it, and it is not so well written an article as I could wish.

I sincerely hope your health is improving.—Believe me yours very faithfully,

ALFRED R. WALLACE.

P.S.—I fear Lubbock's motion is being pushed off to the end of the Session, and Hooker's case will not be fairly considered. I hope the matter will *not* be allowed to drop.—A. R. W.

Down, Beckenham, Kent. August 28, 1872.

My dear Wallace,—I have at last finished the gigantic job of reading Dr. Bastian's book, and have been deeply interested in it. You wished to hear my impression, but it is not worth sending.

He seems to me an extremely able man, as indeed I thought when I read his first essay. His general argument in favour of archebiosis¹ is wonderfully strong; though I cannot think much of some few of his arguments. The result is that I am bewildered and astonished by his statements, but am not convinced; though on the whole it seems to me probable that archebiosis is true. I am not convinced partly I think owing to the deductive cast of much of his reasoning; and I know not why, but I never feel convinced by deduction, even in the case of H. Spencer's writings. If Dr. B.'s book had been turned upside down, and he had begun with the various cases of heterogenesis, and then gone on to organic and afterwards to saline solutions, and had then given his general arguments, I should have been, I believe, much more influenced. I suspect, however, that my chief difficulty is the effect of old convictions being stereotyped on my brain, I must have more evidence that germs or the minutest fragments of the lowest forms are always killed by 212° of Fahr. Perhaps the mere reiteration of the statements given by Dr. B. by other men whose judgment I respect and who have worked long on the lower organisms would suffice to convince me. Here is a fine confession of in-

¹ That is to say, spontaneous generation. For the distinction between archebiosis and heterogenesis, see Bastian, Chap. VI. See also "Life and Letters of Charles Darwin," iii. 168.

tellectual weakness; but what an inexplicable frame of mind is that of belief.

As for Rotifers and Tardigrades being spontaneously generated, my mind can no more digest such statements, whether true or false, than my stomach can digest a lump of lead.

Dr. B. is always comparing archebiosis as well as growth to crystallisation; but on this view a Rotifer or Tardigrade is adapted to its humble conditions of life by a happy accident; and this I cannot believe. That observations of the above nature may easily be altogether wrong is well shown by Dr. B. having declared to Huxley that he had watched the entire development of a leaf of *Sphagnum*. He must have worked with very impure materials in some cases, as plenty of organisms appeared in a saline solution not containing an atom of nitrogen.

I wholly disagree with Dr. B. about many points in his latter chapters. Thus the frequency of generalised forms in the older strata seems to me clearly to indicate the common descent with divergence of more recent forms.

Notwithstanding all his sneers, I do not strike my colours as yet about pangenesis. I should like to live to see archebiosis proved true, for it would be a discovery of transcendent importance; or if false I should like to see it disproved, and the facts otherwise explained; but I shall not live to see all this. If ever proved, Dr. B. will have taken a prominent part in the work. How grand is the onward rush of science; it is enough to console us for the many errors which we have committed and for our efforts being overlaid and forgotten in the mass of new facts and new views which are daily turning up.

This is all I have to say about Dr. B.'s book, and it certainly has not been worth saying. Nevertheless, reward me whenever you can by giving me any news about your appointment to the Bethnal Green Museum.—My dear Wallace, yours very sincerely,

CH. DARWIN.

The Dell, Grays, Essex. August 31, 1872.

Dear Darwin,—Many thanks for your long and interesting letter about Bastian's book, though I almost regret that my asking you for your opinion should have led you to give yourself so much trouble. I quite understand your frame of mind, and

think it quite a natural and proper one. You had hard work to hammer your views into people's heads at first, and if Bastian's theory is true he will have still harder work, because the facts he appeals to are themselves so difficult to establish. Are not you mistaken about the Sphagnum? As I remember it, Huxley detected a fragment of Sphagnum leaf *in the same solution in which a fungoid growth had been developed*. Bastian mistook the Sphagnum also for a vegetable growth, and on account of this ignorance of the character of Sphagnum, and its presence in the solution, Huxley rejected somewhat contemptuously (and I think very illogically) all Bastian's observations. Again, as to the saline solution without nitrogen, would not the air supply what was required?

I quite agree that the book would have gained force by rearrangement in the way you suggest, but perhaps he thought it necessary to begin with a general argument in order to induce people to examine his new collection of facts. I am impressed *most* by the agreement of so many observers, some of whom struggle to explain away their own facts. What a wonderfully ingenious and suggestive paper that is by Galton on "Blood Relationship." It helps to render intelligible many of the eccentricities of heredity, atavism, etc.

Sir Charles Lyell was good enough to write to Lord Ripon and Mr. Cole¹ about me and the Bethnal Green Museum, and the answer he got was that at present no appointment of a director is contemplated. I suppose they see no way of making it a Natural History Museum, and it will have to be kept going by Loan Collections of miscellaneous works of art, in which case, of course, the South Kensington people will manage it. It is a considerable disappointment to me, as I had almost calculated on getting something there.

With best wishes for your good health and happiness, believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

P.S.—I have just been reading Howorth's paper in the *Journal of the Anthropological Institute*. How perverse it is. He throughout confounds "fertility" with "increase of population," which

¹ Sir Henry Cole, K.C.B. (1808-80).

seems to me to be the main cause of his errors. His elaborate accumulation of facts in other papers in *Nature*, on "Subsidence and Elevation of Land," I believe to be equally full of error, and utterly untrustworthy as a whole.—A. R. W.

Down, Beckenham, Kent. September 2, 1872.

My dear Wallace,—I write a line to say that I understood—but I may of course have been mistaken—from Huxley that Bastian distinctly stated that he had watched the development of the scale of *Sphagnum*: I was astonished, as I knew the appearance of *Sphagnum* under a high power, and asked a second time; but I repeat that I may have been mistaken. Busk told me that Sharpey had noticed the appearance of numerous Infusoria in one of the solutions not containing any nitrogen; and I do not suppose that any physiologist would admit the possibility of Infusoria absorbing nitrogen gas. Possibly I ought not to have mentioned statements made in private conversation, so please do not repeat them.

I quite agree about the extreme importance of such men as Cohn [illegible] and Carter having observed apparent cases of heterogenesis. At present I should prefer any mad hypothesis, such as that every disintegrated molecule of the lowest forms can reproduce the parent-form, and that the molecules are universally distributed, and that they do not lose their vital power until heated to such a temperature that they decompose like dead organic particles.

I am extremely grieved to hear about the Museum: it is a great misfortune.—Yours most sincerely,

C. DARWIN.

I have taken up old botanical work and have given up all theories.

I quite agree about Howorth's paper: he wrote to me and I told him that we differed so widely it was of no use our discussing any point.

As for Galton's paper, I have never yet been able to fully digest it: as far as I have, it has not cleared my ideas, and has only aided in bringing more prominently forward the large proportion of the latent characters.

Down, Beckenham, Kent. October 20, 1872.

My dear Wallace,—I have thought that you would perhaps like to see enclosed specimen and extract from letter (translated from the German by my son) from Dr. W. Marshall, Zoological Assistant to Schlegel at Leyden. Neither the specimen nor extract need be returned; and you need not acknowledge the receipt. The resemblance is not so close, now that the fragments are gummed on card, as I at first thought. Your review of Houzeau was very good: I skimmed through the whole gigantic book, but you managed to pick out the plums much better than I did for myself. You are a born critic. What an *admirable* number that was of *Nature*.

I am writing this at Sevenoaks, where we have taken a house for three weeks and have one more week to stay. We came here that I may get a little rest, of which I stood in much need. —Every yours very sincerely,

CH. DARWIN.

With respect to what you say about certain instincts of ants having been acquired by experience or sense, have you kept in mind that the neuters have no progeny? I wish I knew whether the fertile females, or queens, do the same work (viz. placing the eggs in warm places, etc.) as the neuters do afterwards; if so the case would be comparatively simple; but I believe this is not the case, and I am driven to selection of varying pre-existing instincts.

The Dell, Grays, Essex. November 15, 1872.

Dear Darwin,—I should have written earlier to thank you for your book,¹ but was hoping to be able to read more of it before doing so. I have not, however, found time to get beyond the first three chapters, but that is quite sufficient to show me how exceedingly interesting you have made the subject, and how completely and admirably you have worked it out. I expect it will be one of the most popular of your works. I have just been asked to write a review of it for the *Quarterly Journal of Science*, for which purpose I shall be in duty bound to seek out

¹ "Expression of the Emotions."

some deficiencies, however minute, so as to give my notice some flavour of criticism.

The cuts and photos are admirable, and my little boy and girl seized it at once to look at the naughty babies.

With best wishes, believe me yours very faithfully,

ALFRED R. WALLACE.

P.S.—I will take this opportunity of asking you if you know of any book that will give me a complete catalogue of vertebrate fossils with some indication of their affinities.—A. R. W.

Down, Beckenham, Kent. January 13, 1873.

My dear Wallace,—I have read your review with much interest, and I thank you sincerely for the very kind spirit in which it is written. I cannot say that I am convinced by your criticisms.¹ If you have ever actually observed a kitten sucking and pounding with extended toes its mother, and then seen the same kitten when a *little older* doing the same thing on a soft shawl, and ultimately an old cat (as I have seen), and do not admit that it is identically the same action, I am astonished.

With respect to the decapitated frog,² I have always heard of Pflüger as a most trustworthy observer. If, indeed, anyone knows a frog's habits so well as to say that it never rubs off a bit of leaf or other object, which may stick to its thigh, in the same manner as it did the acid, your objection would be valid. Some of Flourens' experiments, in which he removed the cerebral hemisphere from a pigeon, indicate that acts *apparently* performed consciously can be done without consciousness—I presume through the force of habit; in which case it would appear that intellectual power is not brought into play. Several per-

¹ *Quarterly Journal of Science*, January, 1873, p. 116: "I can hardly believe that when a cat, lying on a shawl or other soft material, pats or pounds it with its feet, or sometimes sucks a piece of it, it is the persistence of the habit of pressing the mammary glands and suckling during kittenhood." Wallace goes on to say that infantine habits are generally completely lost in adult life, and that it seems unlikely that they should persist in a few isolated instances.

² Wallace speaks of "a readiness to accept the most marvellous conclusions or interpretations of physiologists on what seem very insufficient grounds," and he goes on to assert that the frog experiment is either incorrectly recorded, or else that it "demonstrates volition, and not reflex action."

sons have made such suggestions and objections as yours about the hands being held up in astonishment:¹ if there was any straining of the muscles, as with protruded arms under fright, I would agree: as it is I must keep to my old opinion, and I daresay you will say that I am an obstinate old blockhead.—My dear Wallace, yours very sincerely, CH. DARWIN.

The book has sold wonderfully; 9,000 copies have now been printed.

The Dell, Grays, Essex. Wednesday morning, [November, 1873].

Dear Darwin,—Yours just received. Pray act exactly as if nothing had been said to me on the subject. I do not particularly *wish* for the work,² as, besides being as you say, tedious work, it involves a considerable amount of responsibility. Still, I am prepared to do any literary work of the kind, as I told Bates some time ago, and that is the reason he wrote to me about it. I certainly think, however, that it would be in many ways more satisfactory to you if your son did it, and I therefore hope he may undertake it.

Should he, however, for any reasons, be unable, I am at your service as a *dernier ressort*.

In case my meaning is not quite clear, I will *not do it* unless your son has the offer and declines it.—Believe me, dear Darwin, yours very faithfully, ALFRED R. WALLACE.

The Dell, Grays, Essex. November 18, 1873.

Dear Darwin,—I quite understand what you require, and would undertake to do it to the best of my ability. Of course in such work I should not think of offering criticisms of matter.

I do not think I could form any idea of how long it would take by seeing the MSS., as it would all depend upon the amount

¹ The raising of the hands in surprise is explained ("Expression of the Emotions," 1st Edit., p. 287) on the doctrine of antithesis as being the opposite of listlessness. Mr. Wallace's view (given in the second edition of "Expression of the Emotions," p. 300) is that the gesture is appropriate to sudden defence or to the giving of aid to another person.

² At this time Darwin, while very busy with other work, had to prepare a second edition of "The Descent of Man," and it is probable that he or the publishers suggested that Wallace should make the necessary corrections.—EDITOR.

of revision and working-in required. I have helped Sir C. Lyell with his last three or four editions in a somewhat similar though different way, and for him I have kept an account simply of the hours I was employed in any way for him, and he paid me 5/- an hour; but (of course this is confidential) I do not think this quite enough for the class of work. I should propose for your work 7/- an hour as a fair remuneration, and I would put down each day the hours I worked at it.

No doubt you will get it done for very much less by any literary man accustomed to regular literary work and nothing else, and perhaps better done, so do not in the least scruple in saying you decide on employing the gentleman you had in view if you prefer it.

If you send it to me could you let me have *all* your MSS. copied out, as it adds considerably to the time required if there is any difficulty in deciphering the writing, which in yours (as you are no doubt aware) there often is.

My hasty note to Bates was not intended to be shown you or anyone. I thought he had heard of it from Murray, and that the arrangement was to be made by Murray.—Believe me
yours very faithfully, ALFRED R. WALLACE.

P.S.—I have been delighted with H. Spencer's "Study of Sociology." Some of the passages in the latter part are *grand*. You have perhaps seen that I am dipping into politics myself occasionally.—A. R. W.

Down, Beckenham, Kent. November 19, 1873.

Dear Wallace,—I thank you for your extremely kind letter, and I am sorry that I troubled you with that of yesterday. My wife thinks that my son George would be so much pleased at undertaking the work for me, that I will write to him, and so probably shall have no occasion to trouble you. If on still further reflection and after looking over my notes, I think that my son could not do the work, I will write again and *gratefully* accept your proposal. But if you do not hear, you will understand that I can manage the affair myself. I never in my life-time regretted an interruption so much as this new edition of the "Descent." I am deeply immersed in some work on physiological points with plants.

I fully agree with what you say about H. Spencer's "Sociology"; I do not believe there is a man in Europe at all his equal in talents. I did not know that you had been writing on politics, except so far as your letter on the coal question, which interested me much and struck me as a capital letter.

I must again thank you for your letter, and remain, dear Wallace, yours very sincerely,
CH. DARWIN.

I hope to Heaven that politics will not replace Natural Science. I know too well how atrociously bad my handwriting is.

The Dell, Grays, Essex. December 6, 1874.

Dear Darwin,—Many thanks for your kindness in sending me a copy of your new edition of the "Descent." I see you have made a whole host of additions and corrections which I shall have great pleasure in reading over as soon as I have got rid of my horrid book on Geographical Distribution, which is almost driving me mad with the amount of drudgery required and the often unsatisfactory nature of the result. However, I must finish with it soon, or all the part first done will have to be done over again, every new book, either as a monograph, or a classification, putting everything wrong (for me).

Hoping you are in good health and able to go on with your favourite work, I remain yours very sincerely,

ALFRED R. WALLACE.

The Dell, Grays, Essex. July 21, 1875.

Dear Darwin,—Many thanks for your kindness in sending me a copy of your new book.¹ Being very busy I have only had time to dip into it yet. The account of *Utricularia* is most marvellous, and quite new to me. I'm rather surprised that you do not make any remarks on the origin of these extraordinary contrivances for capturing insects. Did you think they were too obvious? I daresay there is no difficulty, but I feel sure they will be seized on as inexplicable by Natural Selection, and your silence on the point will be held to show that you consider them so! The contrivance in *Utricularia* and *Dionæa*, and in fact in *Drosera* too, seems fully as great and complex as in

¹ "Insectivorous Plants."

Orchids, but there is not the same motive force. Fertilisation and cross-fertilisation are important ends enough to lead to *any* modification, but can we suppose mere nourishment to be so important, seeing that it is so easily and almost universally obtained by extrusion of roots and leaves. Here are plants which lose their roots and leaves to acquire the same results by infinitely complex modes! What a wonderful and long-continued series of variations must have led up to the perfect "trap" in *Utricularia*, while at any stage of the process the same end might have been gained by a little more development of roots and leaves, as in 9,999 plants out of 10,000!

Is this an imaginary difficulty, or do you mean to deal with it in future editions of the "Origin"? — Believe me yours very faithfully,

ALFRED R. WALLACE.

The Dell, Grays, Essex. November 7, 1875.

Dear Darwin,—Many thanks for your beautiful little volume on "Climbing Plants," which forms a most interesting companion to your "Orchids" and "Insectivorous Plants." I am sorry to see that you have not this time given us the luxury of cut edges.

I am in the midst of printing and proof-sheets, which are wearisome in the extreme from the mass of names and statistics I have been obliged to introduce, and which will, I fear, make my book insufferably dull to all but zoological specialists.

My trust is in my pictures and maps to catch the public.

Hoping yourself and all your family are quite well, believe me yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. June 5, 1876.

My dear Wallace,—I must have the pleasure of expressing to you my unbounded admiration of your book,¹ though I have read only to page 184—my object having been to do as little as possible while resting. I feel sure that you have laid a broad and safe foundation for all future work on Distribution. How interesting it will be to see hereafter plants treated in strict re-

¹ "The Geographical Distribution of Animals," 1876.

lation to your view; and then all insects, pulmonate molluscs, and fresh-water fishes, in greater detail than I suppose you have given to these lower animals. The point which has interested me most, but I do not say the most valuable point, is your protest against sinking imaginary continents in a quite reckless manner, as was started by Forbes, followed, alas, by Hooker, and caricatured by Wollaston and Murray. By the way, the main impression which the latter author has left on my mind is his utter want of all scientific judgment. I have lifted up my voice against the above view with no avail, but I have no doubt that you will succeed, owing to your new arguments and the coloured chart. Of a special value, as it seems to me, is the conclusion that we must determine the areas chiefly by the nature of the mammals. When I worked many years ago on this subject, I doubted much whether the now called Palearctic and Nearctic regions ought to be separated; and I determined if I made another region that it should be Madagascar. I have therefore been able to appreciate the value of your evidence on these points. What progress Palæontology has made during the last 20 years! But if it advances at the same rate in the future, our views on the migration and birthplace of the various groups will, I fear, be greatly altered. I cannot feel quite easy about the Glacial period and the extinction of large mammals, but I much hope that you are right. I think you will have to modify your belief about the difficulty of dispersal of land molluscs; I was interrupted when beginning to experimentise on the just-hatched young adhering to the feet of ground-roosting birds. I differ on one other point, viz. in the belief that there must have existed a Tertiary Antarctic continent, from which various forms radiated to the southern extremities of our present continents. But I could go on scribbling for ever. You have written, as I believe, a grand and memorable work, which will last for years as the foundation for all future treatises on Geographical Distribution.—My dear Wallace, yours very sincerely,

CHARLES DARWIN.

P.S.—You have paid me the highest conceivable compliment by what you say of your work in relation to my chapters on Distribution in the "Origin," and I heartily thank you for it.

The Dell, Grays, Essex. June 7, 1876.

Dear Darwin,—Many thanks for your very kind letter. So few people will read my book at all regularly, that a criticism from one who does so will be very welcome.

If, as I suppose, it is only to p. 184 of Vol. I. that you have read, you cannot yet quite see my conclusions on the points you refer to (land molluscs and Antarctic continent). My own conclusions fluctuated during the progress of the book, and I have, I know, occasionally used expressions (the relics of earlier ideas) which are not quite consistent with what I say further on. I am positively against any Southern continent as *uniting* South America with Australia or New Zealand, as you will see at Vol. I., pp. 398–403 and 459–466. My general conclusions as to Distribution of Land Mollusca¹ are at Vol. II., pp. 522–529. When you have read these passages and looked at the general facts which lead to them, I shall be glad to hear if you still differ from me.

Though, of course, *present results* as to origin and migrations of genera of mammals will have to be modified owing to new discoveries, I cannot help thinking that much will remain unaffected, because in all geographical and geological discoveries the great outlines are soon reached; the details alone remain to be modified. I also think much of the geological evidence is now so accordant with, and explanatory of, geographical distribution that it is *prima facie* correct in outline. Nevertheless, such vast masses of new facts will come out in the next few years that I quite dread the labour of incorporating them in a new edition.

Now for a little personal matter. For two years I have made up my mind to leave this place—mainly for two reasons: drought and wind prevent the satisfactory growth of all delicate plants; and I cannot stand being unable to attend evening meetings and being obliged to refuse every invitation in London. But I was obliged to stay till I had got it into decent order to attract a customer. At last it is so, and I am offering it for sale, and as soon as it is disposed of I intend to try the neighbourhood of

¹ Wallace points out that “hardly a small island on the globe but has some land-shell peculiar to it,” and he goes so far as to say that probably air-breathing mollusca have been chiefly distributed by air- or water-carriage, rather than by voluntary dispersal on the land. See “More Letters,” ii. 14.

Dorking, whence there are late trains from Cannon Street and Charing Cross.

I see your post-mark was Dorking, so I suppose you have been staying there. Is it not a lovely country? I hope your health is improved, and when, quite at your leisure, you have waded through my book, I trust you will again let me have a few lines of friendly criticism and advice.—Yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham. June 17, 1876.

My dear Wallace,—I have now finished the whole of Vol. I., with the same interest and admiration as before; and I am convinced that my judgment was right and that it is a memorable book, the basis of all future work on the subject. I have nothing particular to say, but perhaps you would like to hear my impressions on two or three points. Nothing has struck me more than the admirable and convincing manner in which you treat Java. To allude to a very trifling point, it is capital about the unadorned head of the Argus pheasant.¹ How plain a thing is, when it is once pointed out! What a wonderful case is that of Celebes! I am glad that you have slightly modified your views with respect to Africa,² and this leads me to say that I cannot swallow the so-called continent of Lemuria, i.e. the direct connection of Africa and Ceylon.³ The facts do not seem to me many and strong enough to justify so immense a change of level. Moreover, Mauritius and the other islands

¹ See "The Descent of Man," 1st Edit., pp. 90 and 143, for drawings of the Argus pheasant and its markings. The ocelli on the wing feathers were favourite objects of Darwin's, and sometimes formed the subject of the little lectures which on rare occasions he would give to a visitor interested in Natural History. In Wallace's book, the meaning of the ocelli comes in by the way, in the explanation of Plate IX., "A Malayan Forest with some of its Peculiar Birds." The case is a "remarkable confirmation of Mr. Darwin's views, that gaily coloured plumes are developed in the male bird for the purpose of attractive display."

² "Geographical Distribution of Animals," i. 286-7.

³ "Geographical Distribution," i. 76. The name Lemuria was proposed by Mr. Sclater for an imaginary submerged continent extending from Madagascar to Ceylon and Sumatra. Wallace points out that if we confine ourselves to facts Lemuria is reduced to Madagascar, which he makes a subdivision of the Ethiopian Region.

appear to me oceanic in character. But do not suppose that I place my judgment on this subject on a level with yours. A wonderfully good paper was published about a year ago on India in the *Geological Journal*—I *think* by Blandford.¹ Ramsay agreed with me that it was one of the best published for a long time. The author shows that India has been a continent with enormous fresh-water lakes from the Permian period to the present day. If I remember right he believes in a former connection with South Africa.

I am sure that I read, some 20 to 30 years ago, in a French journal, an account of teeth of Mastodon found in Timor; but the statement may have been an error.

With respect to what you say about the colonising of New Zealand, I somewhere have an account of a frog frozen in the ice of a Swiss glacier, and which revived when thawed. I may add that there is an Indian toad which can resist salt water and haunts the seaside. Nothing ever astonished me more than the case of the Galaxias; but it does not seem known whether it may not be a migratory fish like the salmon. It seems to me that you complicate rather too much the successive colonisations with New Zealand. I should prefer believing that the Galaxias was a species, like the Emys of the Sewalik Hills, which has long retained the same form. Your remarks on the insects and flowers of New Zealand have greatly interested me; but aromatic leaves I have always looked at as a protection against their being eaten by insects or other animals; and as insects are there rare, such protection would not be much needed. I have written more than I intended, and I must again say how profoundly your book has interested me.

Now let me turn to a very different subject. I have only just heard of and procured your two articles in the *Academy*. I thank you most cordially for your generous defence of me against Mr. Mivart. In the "Origin" I did not discuss the derivation of any one species; but that I might not be accused of concealing my opinion I went out of my way and inserted a sentence which seemed to me (and still so seems) to declare plainly my belief. This was quoted in my "Descent of Man."

¹ H. F. Blandford, "On the Age and Correlations of the Plant-bearing Series of India and the Former Existence of an Indo-Oceanic Continent" (*Quart. Journ. Geol. Soc.*, 1875, xxi. 519).

Therefore it is very unjust, not to say dishonest, of Mr. Mivart to accuse me of base fraudulent concealment; I care little about myself; but Mr. Mivart, in an article in the *Quarterly Review* (which I know was written by him), accused my son George of encouraging profligacy, and this without the least foundation.¹ I can assert this positively, as I laid George's article and the *Quarterly Review* before Hooker, Huxley and others, and all agreed that the accusation was a deliberate falsification. Huxley wrote to him on the subject and has almost or quite cut him in consequence; and so would Hooker, but he was advised not to do so as President of the Royal Society. Well, he has gained his object in giving me pain, and, good God, to think of the flattering, almost fawning speeches which he has made to me! I wrote, of course, to him to say that I would never speak to him again. I ought, however, to be contented, as he is the one man who has ever, as far as I know, treated me basely.

Forgive me for writing at such length and believe me yours
very sincerely,
CH. DARWIN.

¹ In the *Contemporary Review* for August, 1873, Mr. George Darwin wrote an article "On Beneficial Restrictions to Liberty of Marriage." In the July number of the *Quarterly Review*, 1874, p. 70, in an article entitled "Primitive Man—Tylor and Lubbock," Mr. Mivart thus referred to Mr. Darwin's article: "Elsewhere (pp. 424-5) Mr. George Darwin speaks (1) in an approving strain of the most oppressive laws and of the encouragement of vice to check population. (2) There is no sexual criminality of Pagan days that might not be defended on the principles advocated by the school to which this writer belongs." In the *Quarterly Review* for October, 1874, p. 587, appeared a letter from Mr. George Darwin "absolutely denying" charge No. 1, and with respect to charge No. 2 he wrote: "I deny that there is any thought or word in my essay which could in any way lend itself to the support of the nameless crimes here referred to." To the letter was appended a note from Mr. Mivart, in which he said: "Nothing would have been further from our intention than to tax Mr. Darwin personally (as he seems to have supposed) with the advocacy of laws or acts which he saw to be oppressive or vicious. We, therefore, most willingly accept his disclaimer, and are glad to find that he does not, in fact, apprehend the full tendency of the doctrines which he has helped to propagate. Nevertheless, we cannot allow that we have enunciated a single proposition which is either 'false' or 'groundless.' . . . But when a writer, according to his own confession, comes before the public 'to attack the institution of marriage' . . . he must expect searching criticism; and, without implying that Mr. Darwin has in 'thought' or 'word' approved of anything which he wishes to disclaim, we must still maintain that the doctrines which he advocates are most dangerous and pernicious."—EDITOR.

P.S.—I am very sorry that you have given up sexual selection. I am not at all shaken, and stick to my colours like a true Briton. When I think about the unadorned head of the Argus pheasant, I might exclaim, *Et tu, Brute!*

Down, Beckenham. June 25, 1876.

My dear Wallace,—I have been able to read rather more quickly of late and have finished your book. I have not much to say. Your careful account of the temperate parts of South America interested me much, and all the more from knowing something of the country. I like also much the general remarks towards the end of the volume on the land molluscs. Now for a few criticisms.

P. 122:¹ I am surprised at your saying that "during the whole Tertiary period North America was zoologically far more strongly contrasted with South America than it is now." But we know hardly anything of the latter except during the Pliocene period, and then the mastodon, horse, several great *Dentata*, etc., etc., were common to the North and South. If you are right I erred greatly in my Journal, where I insisted on the former close connection between the two.

P. 252, and elsewhere: I agree thoroughly with the general principle that a great area with many competing forms is necessary for much and high development; but do you not extend this principle too far—I should say much too far, considering how often several species of the same genus have been developed on very small islands?

P. 265: You say that the Sittidæ extend to Madagascar, but there is no number in the tabular heading.²

P. 359: *Rhinochetus* is entered in the tabular heading under No. 3 of the *Neotropical* sub-regions.³

Reviewers think it necessary to find some fault, and if I were to review you, the sole point which I should blame is your not giving very numerous references. These would save whoever follows you great labour. Occasionally I wished myself to know the authority for certain statements, and whether you or somebody else had originated certain subordinate views. Take the

¹ The pages refer to Vol. II. of Wallace's "Geographical Distribution."

² The number (4) was erroneously omitted.—A. R. W.

³ An error: should have been the Australian.—A. R. W.

case of a man who had collected largely on some island, for instance St. Helena, and who wished to work out the geographical relations of his collection; he would, I think, feel very blank at not finding in your work precise references to all that had been written on St. Helena. I hope you will not think me a confoundedly disagreeable fellow.

I may mention a capital essay which I received a few months ago from Axel Blytt¹ on the distribution of the plants of Scandinavia; showing the high probability of there having been secular periods alternately wet and dry; and of the important part which they have played in distribution.

I wrote to Forel, who is always at work on ants, and told him of your views about the dispersal of the blind Coleoptera, and asked him to observe.

I spoke to Hooker about your book, and feel sure that he would like nothing better than to consider the distribution of plants in relation to your views; but he seemed to doubt whether he should ever have time.

And now I have done my jottings, and once again congratulate you on having brought out so grand a work. I have been a little disappointed at the review in *Nature*.²—My dear Wallace, yours sincerely,

CHARLES DARWIN.

Rose Hill, Dorking. July 23, 1876.

My dear Darwin,—I should have replied sooner to your last kind and interesting letters, but they reached me in the midst of my packing previous to removal here, and I have only just now got my books and papers in a get-at-able state.

And first, many thanks for your close observation in detecting the two absurd mistakes in the tabular headings.

As to the former greater distinction of the North and South American faunas, I think I am right. The Edentata being proved (as I hold) to have been mere temporary migrants into North America in the post-Pliocene epoch, form no part of its Tertiary fauna. Yet in South America they were so enormously developed in the Pliocene epoch that we know, if there is any

¹ Axel Blytt, "Essay on the Immigration of the Norwegian Flora." Christiania, 1876.

² June 22, 1876, p. 165 *et seq.*

such thing as Evolution, etc., that strange ancestral forms must have preceded them in Miocene times.

Mastodon, on the other hand, represented by one or two species only, appears to have been a late immigrant into South America from the North.

The immense development of Ungulates (in varied families, genera, and species) in North America during the whole Tertiary epoch is, however, the great feature, which assimilates it to Europe and contrasts it with South America. True camels, hosts of hog-like animals, true rhinoceroses, and hosts of ancestral horses, all bring North America much nearer to the Old World than it is now. Even the horse, represented in all South America by *Equus* only, was probably a temporary immigrant from the North.

As to extending too far the principle (yours) of the necessity of comparatively large areas for the development of varied faunas, I may have done so, but I think not. There is, I think, every probability that most islands, etc., where a varied fauna now exists have been once more extensive, e.g. New Zealand, Madagascar. Where there is no such evidence (e.g. Galapagos), the fauna is *very restricted*.

Lastly as to want of references; I confess the justice of your criticism. But I am dreadfully unsystematic. It is my first large work involving much of the labour of others. I began with the intention of writing a comparatively short sketch, enlarged it, and added to it, bit by bit; remodelled the tables, the headings, and almost everything else, more than once, and got my materials into such confusion that it is a wonder it has not turned out far more crooked and confused than it is. I, no doubt, ought to have given references; but in many cases I found the information so small and scattered, and so much had to be combined and condensed from conflicting authorities, that I hardly knew how to refer to them or where to leave off. Had I referred to all authors consulted for every fact, I should have greatly increased the bulk of the book, while a large portion of the references would be valueless in a few years owing to later and better authorities. My experience of referring to references has generally been most unsatisfactory. One finds, nine times out of ten, the fact is stated, and nothing more; or a reference to some third work not at hand!

I wish I could get into the habit of giving chapter and verse for every fact and extract, but I am too lazy and generally in a hurry, having to consult books against time when in London for a day.

However, I will try and do something to mend this matter should I have to prepare another edition.

I return you Forel's letter. It does not advance the question much, neither do I think it likely that even the complete observation he thinks necessary would be of much use; because it may well be that the ova or larvæ or imagos of the beetles are not carried systematically by the ants, but only occasionally owing to some exceptional circumstances. This might produce a great effect in distribution, yet be so rare as never to come under observation.

Several of your remarks in previous letters I shall carefully consider. I know that, compared with the extent of the subject, my book is in many parts crude and ill-considered; but I thought, and still think, it better to make *some generalisations* wherever possible, as I am not at all afraid of having to alter my views in many points of detail. I was so overwhelmed with zoological details that I never went through the Geological Society's *Journal* as I ought to have done, and as I mean to do before writing more on the subject.

With best wishes, believe me yours very faithfully,
ALFRED R. WALLACE.

Rose Hill, Dorking. December 13, 1876.

My dear Darwin,—Many thanks for your new book on "Crossing Plants," which I have read with much interest. I hardly expected, however, that there would have been so many doubtful and exceptional cases. I fancy that the results would have come out better had you always taken weights instead of heights; and that would have obviated the objection that will, I daresay, be made, that *height* proves nothing, because a tall plant may be weaker, less bulky and less vigorous than a shorter one. Of course no one who knows you or who takes a *general* view of your results will say this, but I daresay it will be said. I am afraid this book will not do much or anything to get rid of the one great objection, that the physiological characteristic of species, the infertility of hybrids, has not yet been produced.

Have you ever tried experiments with plants (if any can be found) which for several centuries have been grown under very different conditions, as for instance potatoes on the high Andes and in Ireland? If any approach to sterility occurred in mongrels between these it would be a grand step. The most curious point you have brought out seems to me the slight superiority of self-fertilisation over fertilisation with another flower of the same plant, and the most important result, that difference of constitution is the essence of the benefit of cross-fertilisation. All you now want is to find the neutral point where the benefit is at its maximum, any greater difference being prejudicial.

Hoping you may yet demonstrate this, believe me yours very faithfully,

ALFRED R. WALLACE.

Rose Hill, Dorking. January 17, 1877.

My dear Darwin,—Many thanks for your valuable new edition of the "Orchids," which I see contains a great deal of new matter of the greatest interest. I am amazed at your continuous work, but I suppose, after all these years of it, it is impossible for you to remain idle. I, on the contrary, am very idle, and feel inclined to do nothing but stroll about this beautiful country, and read all kinds of miscellaneous literature.

I have asked my friend Mr. Mott to send you the last of his remarkable papers—on Haeckel. But the part I hope you will read with as much interest as I have done is that on the deposits of Carbon, and the part it has played and must be playing in geological changes. He seems to have got the idea from some German book, but it seems to me very important, and I wonder it never occurred to Sir Charles Lyell. If the calculations as to the quantity of undecomposed carbon deposited are anything approaching to correctness, the results must be important.

Hoping you are in pretty good health, believe me yours very faithfully,

ALFRED R. WALLACE.

Rose Hill, Dorking. July 23, 1877.

My dear Darwin,—Many thanks for your admirable volume on "The Forms of Flowers." It would be impertinence of me to say anything in praise of it, except that I have read the chap-

ters on "Illegitimate Offspring of Heterostyled Plants" and on "Cleistogamic Flowers" with great interest.

I am almost afraid to tell you that in going over the subject of the Colours of Animals, etc., for a small volume of essays, etc., I am preparing, I have come to conclusions directly opposed to *voluntary sexual selection*, and believe that I can explain (in a general way) *all* the phenomena of sexual ornaments and colours by laws of development aided by simple Natural Selection.

I hope you admire as I do Mr. Belt's remarkable series of papers in support of his terrific "oceanic glacier river-damming" hypothesis. In awful grandeur it beats everything "glacial" yet out, and it certainly explains a wonderful lot of hard facts. The last one, on the "Glacial Period in the Southern Hemisphere," in the *Quarterly Journal of Science*, is particularly fine, and I see he has just read a paper at the Geological Society. It seems to me supported by quite as much evidence as Ramsay's "Lakes"; but Ramsay, I understand, will have none of it—as yet.—Believe me yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. August 31, 1877.

My dear Wallace,—I am very much obliged to you for sending your article, which is very interesting and appears to me as clearly written as it can be. You will not be surprised that I differ altogether from you about sexual colours. That the tail of the peacock and his elaborate display of it should be due merely to the vigour, activity, and vitality of the male is to me as utterly incredible as my views are to you. Mantegazza published a few years ago in Italy a somewhat similar view. I cannot help doubting about recognition through colour: our horses, dogs, fowls, and pigeons seem to know their own species, however differently the individuals may be coloured. I wonder whether you attribute the odoriferous and sound-producing organs, when confined to the males, to their greater vigour, etc. I could say a good deal in opposition to you, but my arguments would have no weight in your eyes, and I do not intend to write for the public anything on this or any other difficult subject. By the way, I doubt whether the term *voluntary* in relation to sexual selection ought to be employed: when a man is fascinated

by a pretty girl it can hardly be called voluntary, and I suppose that female animals are charmed or excited in nearly the same manner by the gaudy males.

Three essays have been published lately in Germany which would interest you: one by Weismann, who shows that the coloured stripes on the caterpillars of Sphinx are beautifully protective: and birds were frightened away from their feeding-place by a caterpillar with large eye-like spots on the broad anterior segments of the body. Fritz Müller has well discussed the first steps of mimicry with butterflies, and comes to nearly or quite the same conclusion as you, but supports it by additional arguments.

Fritz Müller also has lately shown that the males alone of certain butterflies have odoriferous glands on their wings (distinct from those which secrete matter disgusting to birds), and where these glands are placed the scales assume a different shape, making little tufts.

Farewell: I hope that you find Dorking a pleasant place? I was staying lately at Abinger Hall, and wished to come over to see you, but driving tires me so much that my courage failed.
—Yours very sincerely,

CHARLES DARWIN.

Madeira Villa, Madeira Road, Ventnor, Isle of Wight.

September 3, 1877.

My dear Darwin,—Many thanks for your letter. Of course I did not expect my paper to have any effect on your opinions. You have looked at all the facts so long from your special point of view that it would require conclusive arguments to influence you, and these, from the complex nature of the question, are probably not to be had. We must, I think, leave the case in the hands of others, and I am in hopes that my paper may call sufficient attention to the subject to induce some of the great school of Darwinians to take the question up and work it out thoroughly. You have brought such a mass of facts to support your view, and have argued it so fully, that I hardly think it necessary for you to do more. Truth will prevail, as you as well as I wish it to do. I will only make one or two remarks. The word “voluntary” was inserted in *my proofs only*, in order to distinguish clearly between the two radically distinct kinds of

"sexual selection." Perhaps "conscious" would be a better word, to which I think you will not object, and I will alter it when I republish. I lay no stress on the word "voluntary."

Sound- and scent-producing organs in males are surely due to "natural" or "automatic" as opposed to "conscious" selection. If there were gradations in the sounds produced, from mere noises, up to elaborate music—the case would be analogous to that of "colours" and "ornament." Being, however, comparatively simple, Natural Selection, owing to their use as a guide, seems sufficient. The louder sound, heard at a greater distance, would attract or be heard by more females, or it may attract other males and lead to combats *for* the females, but this would not imply *choice* in the sense of rejecting a male whose stridulation was a trifle less loud than another's, which is the essence of the theory as applied by you to colour and ornament. But greater general vigour would almost certainly lead to greater volume or persistence of sound, and so the same view will apply to both cases on my theory.

Thanks for the references you give me. My ignorance of German prevents me supporting my views by the mass of observations continually being made abroad, so I can only advance my own ideas for what they are worth.

I like Dorking much, but can find no house to suit me, so fear I shall have to move again.

With best wishes, believe me yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. September 5, [1877].

My dear Wallace,—"Conscious" seems to me much better than "voluntary." Conscious action, I presume, comes into play when two males fight for a female; but I do not know whether you admit that, for instance, the spur of the cock is due to sexual selection.

I am quite willing to admit that the sounds and vocal organs of some males are used only for challenging, but I doubt whether this applies to the musical notes of hylobates or to the howling (I judge chiefly from Rengger) of the American monkeys. No account that I have seen of the stridulation of male insects shows that it is a challenge. All those who have attended to birds consider their song as a charm to the females and not as

a challenge. As the males in most cases search for the females I do not see how their odoriferous organs will aid them in finding the females. But it is foolish in me to go on writing, for I believe I have said most of this in my book: anyhow, I well remember thinking over it. The "belling" of male stags, if I remember rightly, is a challenge, and so I daresay is the roaring of the lion during the breeding season.

I will just add in reference to your former letter that I fully admit that with birds the fighting of the males co-operates with their charms; and I remember quoting Bartlett that gaudy colouring in the males is almost invariably concomitant with pugnacity. But, thank Heaven, what little more I can do in science will be confined to observation on simple points. However much I may have blundered, I have done my best, and that is my constant comfort.—Most truly yours,

C. DARWIN.

Waldron Edge, Duppas Hill, Croydon. September 14, 1878.

Dear Darwin,—An appointment is soon to be made of someone to have the superintendence of Epping Forest under the new Act, and as it is a post which of all others I should like I am trying very hard to get up interest enough to secure it.

One of the means is the enclosed memorial, which has been already signed by Sir J. Hooker and Sir J. Lubbock, and to which I feel sure you will add your name, which I expect has weight "even in the City."

In want of anything better to do I have been grinding away at a book on the Geography of Australia for Stanford for the last six months.

Hoping you are in good health and with my best compliments to Mrs. Darwin and the rest of your family, believe me yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. September 16, 1878.

My dear Wallace,—I return the paper signed, and most heartily wish that you may be successful, not only for your own sake, but for that of Natural Science, as you would then have more time for new researches.

I keep moderately well, but always feel half-dead, yet manage

to work away on vegetable physiology, as I think that I should die outright if I had nothing to do.—Believe me yours very sincerely,

CH. DARWIN.

Waldron Edge, Duppas Hill, Croydon. September 23, 1878.

Dear Darwin,—Many thanks for your signature and good wishes. I have some hopes of success, but am rather doubtful of the Committee of the Corporation who will have the management, for they have just decided after a great struggle in the Court of Common Council that it is to be a rotatory Committee, every member of the Council (of whom there are 200) coming on it in succession if they please. They evidently look upon it as a Committee which will have great opportunities of excursions, picnics, and dinners, at the expense of the Corporation, while the improvement of the Forest will be quite a secondary matter.

I am very glad to hear you are tolerably well. It is all I can say of myself.—Believe me yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. January 5, 1880.

My dear Wallace,—As this note requires no sort of answer, you must allow me to express my lively admiration of your paper in the *Nineteenth Century*.¹ You certainly are a master in the difficult art of clear exposition. It is impossible to urge too often that the selection from a single varying individual or of a single varying organ will not suffice. You have worked in capitally Allen's admirable researches. As usual, you delight to honour me more than I deserve. When I have written about the extreme slowness of Natural Selection (in which I hope I may be wrong), I have chiefly had in my mind the effects of intercrossing. I subscribe to almost everything you say excepting the last short sentence.

And now let me add how grieved I was to hear that the City of London did not elect you for the Epping office, but I suppose it was too much to hope that such a body of men should make a good selection. I wish you could obtain some quiet post and

¹ "The Origin of Species and Genera."

thus have leisure for moderate scientific work. I have nothing to tell you about myself; I see few persons, for conversation fatigues me much; but I daily do some work in experiments on plants, and hope thus to continue to the end of my days.

With all good wishes, believe me yours very sincerely,

CHARLES DARWIN.

P.S.—Have you seen Mr. Farrer's article in the last *Fortnightly*? It reminded me of an article on bequests by you some years ago which interested and almost converted me.

Waldron Edge, Duppas Hill, Croydon. January 9, 1880.

My dear Darwin,—It is a great pleasure to receive a letter from you sometimes—especially when we do not differ very much. I am, of course, much pleased and gratified that you like my article. I wrote it chiefly because I thought there was something a little fresh still to say on the subject, and also because I wished to define precisely my present position, which people continually misunderstand. The main part of the article forms part of a chapter of a book I have now almost finished on my favourite subject of “Geographical Distribution.” It will form a sort of supplement to my former work, and will, I trust, be more readable and popular. I go pretty fully into the laws of variation and dispersal; the exact character of specific and generic areas, and their causes; the growth, dispersal and extinction of species and groups, illustrated by maps, etc.; changes of geography and of climate as affecting dispersal, with a full discussion of the Glacial theory, adopting Croll's views (part of this has been published as a separate article in the *Quarterly Review* of last July, and has been highly approved by Croll and Geikie); a discussion of the theory of permanent continents and oceans, which I see you were the first to adopt, but which geologists, I am sorry to say, quite ignore. All this is preliminary. Then follows a series of chapters on the different kinds of islands, continental and oceanic, with a pretty full discussion of the characters, affinities, and origin of their fauna and flora in typical cases. Among these I am myself quite pleased with my chapters on New Zealand, as I believe I have fully explained and accounted for *all* the main peculiarities of the New Zealand

and Australian floras. I call the book "Island Life," etc., etc., and I think it will be interesting.

Thanks for your regrets and kind wishes anent Epping. It was a disappointment, as I had good friends on the Committee and therefore had too much hope. I may just mention that I am thinking of making some application through friends for some post in the new Josiah Mason College of Science at Birmingham, as Registrar or Curator and Librarian, etc. The Trustees have advertised for Professors to begin next October. Should you happen to know any of the Trustees, or have any influential friends in Birmingham, perhaps you could help me.

I think this book will be my last, as I have pretty well said all I have to say in it, and I have never taken to experiment as you have. But I want some easy occupation for my declining years, with not too much confinement or desk-work, which I cannot stand. You see I had some reason for writing to you; but do not you trouble to write again unless you have something to communicate.

With best wishes, yours very faithfully,

ALFRED R. WALLACE.

I have not seen the *Fortnightly* yet, but will do so.

Pen-y-bryn, St. Peter's Road, Croydon. October 11, 1880.

My dear Darwin,—I hope you will have received a copy of my last book, "Island Life," as I shall be very glad of your opinion on certain points in it. The first five chapters you need not read, as they contain nothing fresh to you, but are necessary to make the work complete in itself. The next five chapters, however (VII. to X.), I think, will interest you. As I *think*, in Chapters VIII. and IX. I have found the true explanation of geological climates, and on this I shall be very glad of your candid opinion, as it is the very foundation-stone of the book. The rest will not contain much that is fresh to you, except the three chapters on New Zealand. Sir Joseph Hooker thinks my theory of the Australian and New Zealand floras a decided advance on anything that has been done before.

In connection with this, the chapter on the Azores should be read.

Chap. XVI. on the British Fauna may also interest you.

I mention these points merely that you may not trouble yourself to read the whole book, unless you like.

Hoping that you are well, believe me yours very faithfully,
ALFRED R. WALLACE.

Down, Beckenham, Kent. November 3, 1880.

My dear Wallace,—I have now read your book,¹ and it has interested me deeply. It is quite excellent, and seems to me the best book which you have ever published; but this may be merely because I have read it last. As I went on, I made a few notes,² chiefly when I differed strongly from you; but God knows whether they are worth your reading. You will be disappointed with many of them; but they will show that I had the will, though I did not know the way, to do what you wanted.

I have said nothing on the infinitely many passages and views which I admired and which were new to me. My notes are badly expressed; but I thought that you would excuse my taking any pains with my style. I wish that my confounded handwriting was better.

I had a note the other day from Hooker, and I can see that he is *much* pleased with the Dedication.

With all good wishes, believe me yours sincerely,

CH. DARWIN.

¹ "Island Life."

² In "My Life" (ii. 12-13) Wallace writes: "With this came seven foolscap pages of notes, many giving facts from his extensive reading which I had not seen. There were also a good many doubts and suggestions on the very difficult questions in the discussion of the causes of the glacial epochs. Chapter XXIII., discussing the Arctic element in South Temperate floras, was the part he most objected to, saying, 'This is rather too speculative for my old noddle. I must think that you overrate the importance of new surfaces on mountains and dispersal from mountain to mountain. I still believe in Alpine plants having lived on the lowlands and in the southern tropical regions having been cooled during glacial periods, and thus only can I understand character of floras on the isolated African mountains. It appears to me that you are not justified in arguing from dispersal to oceanic islands to mountains. Not only in latter cases currents of sea are absent, but what is there to make birds fly direct from one Alpine summit to another? There is left only storms of wind, and if it is probable or possible that seeds may thus be carried for great distances, I do not believe that there is at present any evidence of their being thus carried more than a few miles.' This is the most connected piece of criticism in the notes, and I therefore give it verbatim."

In two or three weeks you will receive a book from me; if you care to know what it is about, read the paragraph in Introduction about new terms and then the last chapter, and you will know whole contents of book.

Pen-y-bryn, St. Peter's Road, Croydon. November 8, 1880.

My dear Darwin,—Many thanks for your kind remarks and notes on my book. Several of the latter will be of use to me if I have to prepare a second edition, which I am not so sure of as you seem to be.

1. In your remark as to the doubtfulness of paucity of fossils being due to coldness of water, I think you overlook that I am speaking *only* of waters in the latitude of the Alps, in Miocene and Eocene times, when icebergs and glaciers temporarily descended into an otherwise warm sea; my theory being that there was no glacial epoch at that time, but merely a local and temporary descent of the snow-line and glaciers owing to high excentricity and winter in *aphelion*.

2. I cannot see the difficulty about the cessation of the glacial period. Between the Miocene and the Pleistocene periods geographical changes occurred which rendered a true glacial period possible with high excentricity. When the high excentricity passed away the glacial epoch also passed away in the Temperate zone; but it persists in the Arctic zone, where during the Miocene there were mild climates, and this is due to the persistence of the changed geographical conditions. The present Arctic climate is itself a comparatively new and abnormal state of things due to geographical modification. As to "epoch" and "period," I use them as synonyms to avoid repeating the same word.

3. Rate of deposit and geological time: there no doubt I may have gone to an extreme, but my "twenty-eight million years" may be anything under 100 millions, as I state. There is an enormous difference between *mean* and *maximum* denudation and deposition. In the case of the great faults the upheaval along a given line would itself facilitate the denudation (whether subaerial or marine) of the upheaved portion at a rate perhaps a hundred times faster than plains and plateaux. So, local subsidence might itself lead to very rapid deposition. Suppose a portion of the Gulf of Mexico near the mouth of the Mississippi

were to subside for a few thousand years, it might receive the greater part of the sediment from the whole Mississippi valley and thus form strata at a very rapid rate.

4. You quote the Pampas thistles, etc., against my statement of the importance of preoccupation. But I am referring especially to St. Helena, and to plants naturally introduced from the adjacent continents. Surely, if a certain number of African plants reached the island and became modified into a complete adaptation to its climatic conditions, they would hardly be expelled by other African plants arriving subsequently. They might be so conceivably, but it does not seem probable. The cases of the Pampas, New Zealand, Tahiti, etc., are very different, where highly developed *aggressive* plants have been artificially introduced. Under nature it is these very aggressive species that would first reach any island in their vicinity, and, being adapted to the island and colonising it thoroughly, would then hold their own against other plants from the *same* country, mostly less aggressive in character. I have not explained this so fully as I should have done in the book. Your criticism is therefore useful.

My Chap. XXIII. is no doubt very speculative, and I cannot wonder at your hesitating at accepting my views. To me, however, your theory of hosts of existing species migrating over the tropical lowlands from the North Temperate to the South Temperate zone appears more speculative and more improbable. For, where could the rich lowland *equatorial* flora have existed during a period of general refrigeration sufficient for this? and what became of the wonderfully rich Cape flora which, if the temperature of Tropical Africa had been so recently lowered, would certainly have spread northwards and on the return of the heat could hardly have been driven back into the sharply defined and *very restricted area* in which it now exists?

As to the migration of plants from mountain to mountain not being so probable as to remote islands, I think that is fully counterbalanced by two considerations:

(a) The area and abundance of the mountain stations along such a range as the Andes are immensely greater than those of the islands in the North Atlantic, for example.

(b) The temporary occupation of mountain stations by migrating plants (which I think I have shown to be probable)

renders *time* a much more important element in increasing the number and variety of the plants so dispersed than in the case of islands, where the flora soon acquires a fixed and endemic character, and where the number of species is necessarily limited.

No doubt, direct evidence of seeds being carried great distances through the air is wanted, but, I am afraid, can hardly be obtained. Yet I feel the greatest confidence that they *are* so carried. Take for instance the two peculiar orchids of the Azores (*Habınaria* species): what other mode of transit is conceivable? The whole subject is one of great difficulty, but I hope my chapter may call attention to a hitherto neglected factor in the distribution of plants.

Your references to the Mauritius literature are very interesting, and will be useful to me; and again thanking you for your valuable remarks, believe me yours very faithfully,

ALFRED R. WALLACE.

Pen-y-bryn, St. Peter's Road, Croydon. November 21, 1880.

My dear Darwin,—Many thanks for your new book containing your wonderful series of experiments and observations on the movements of plants. I have read the introduction and conclusion, which shows me the importance of the research as indicating the common basis of the infinitely varied habits and mode of growth of plants. The whole subject becomes thus much simplified, though the nature of the basic vitality which leads to such wonderful results remains as mysterious as ever.—Yours very faithfully,

ALFRED R. WALLACE.

Pen-y-bryn, St. Peter's Road, Croydon. January 1, 1881.

My dear Darwin,—I have been intending to write to you for some weeks to call your attention to what seems to me a striking confirmation (or at all events a support) of my views of the land migration of plants from mountain to mountain. In *Nature* of Dec. 9th, p. 126, Mr. Baker, of Kew, describes a number of the alpine plants of Madagascar as being *identical species* with some found on the mountains of Abyssinia, the Cameroons, and other African mountains. Now, if there is one thing more clear than another it is that Madagascar has been separated from

Africa since the Miocene (probably the early Miocene) epoch. These plants must therefore have reached the island either *since* then, in which case they certainly must have passed through the air for long distances, or at the time of the union. But the Miocene and Eocene periods were certainly warm, and these alpine plants could hardly have migrated over tropical forest lands, while it is very improbable that if they had been isolated at so remote a period, exposed to such distinct climatal and organic environments as in Madagascar and Abyssinia, they would have in both places retained their specific characters unchanged. The presumption is, therefore, that they are comparatively *recent* immigrants, and if so must have passed across the sea from mountain to mountain, for the richness and speciality of the Madagascar forest vegetation render it certain that no recent glacial epoch has seriously affected that island.

Hoping that you are in good health and wishing you the compliments of the season, I remain yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. January 2, 1881.

My dear Wallace,—The case which you give is a very striking one, and I had overlooked it in *Nature*.¹ But I remain as great a heretic as ever. Any supposition seems to me more probable than that the seeds of plants should have been blown from the mountains of Abyssinia or other central mountains of Africa to the mountains of Madagascar. It seems to me almost infinitely more probable that Madagascar extended far to the south during the Glacial period, and that the southern hemisphere was, according to Croll, then more temperate; and that the whole of Africa was then peopled with some temperate forms, which crossed chiefly by agency of birds and sea-currents; and some few by the wind from the shores of Africa to Madagascar, subsequently ascending to the mountains.

How lamentable it is that two men should take such widely different views, with the same facts before them; but this seems to be almost regularly our case, and much do I regret it.

I am fairly well, but always feel half dead with fatigue. I

¹ “*Nature*, December 9, 1880. The substance of this article by Mr. Baker, of Kew, is given in ‘More Letters,’ vol. iii. 25, in a footnote.”—“My Life,” ii. 13.

heard but an indifferent account of your health some time ago, but trust that you are now somewhat stronger.—Believe me, my dear Wallace, yours very sincerely,

CH. DARWIN.

Down, Beckenham, Kent. January 7, 1881.

My dear Wallace,—You know from Miss Buckley that, with her assistance, I drew up a memorial to Mr. Gladstone with respect to your services to science. The memorial was corrected by Huxley, who has aided me in every possible way. It was signed by twelve good men, and you would have been gratified if you had seen how strongly they expressed themselves on your claims.

The Duke of Argyll, to whom I sent the memorial, wrote a private note to Mr. Gladstone. The memorial was sent in only on January 5th, and I have just received a note in Mr. Gladstone's own handwriting, in which he says: "I lose no time in apprising you that although the Fund is moderate and at present poor, I shall recommend Mr. Wallace for a pension of £200 a year." I will keep this note carefully, as, if the present Government were to go out, I do not doubt that it would be binding on the next Government.

I hope that it will give you some satisfaction to see that not only every scientific man to whom I applied, but that also our Government appreciated your lifelong scientific labour.—Believe me, my dear Wallace, yours sincerely,

CH. DARWIN.

I should expect that there will be some delay before you receive an official announcement.

Pen-y-bryn, St. Peter's Road, Croydon. January 8, 1881.

My dear Darwin,—I need not say how very grateful I am to you for your constant kindness, and especially for the trouble you have taken in recommending me to Mr. Gladstone. It is also, of course, very gratifying to hear that so many eminent men have so good an opinion of the little scientific work I have done, for I myself feel it to be very little in comparison with that of many others.

The amount you say Mr. Gladstone proposes to recommend

is considerably more than I expected would be given, and it will relieve me from a great deal of the anxieties under which I have laboured for several years. To-day is my fifty-eighth birthday, and it is a happy omen that your letter should have arrived this morning.

I presume after I receive the official communication will be the proper time to thank the persons who have signed the memorial in my favour. I do not know whether it is the proper etiquette to write a private letter of thanks to Mr. Gladstone, or only a general official one. Whenever I hear anything from the Government I will let you know.

Again thanking you for your kindness, believe me yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. January 10, 1881.

My dear Wallace,—I am heartily glad that you are pleased about the memorial.

I do not feel that my opinion is worth much on the point which you mention. A relation who is in a Government office and whose judgment, I think, may be fully trusted, felt sure that if you received an official announcement without any private note, it ought to be answered officially, but if the case were mine, I would express whatever I thought and felt in an official document. His reason was that Gladstone gives or recommends the pension on public grounds alone.

If the case were mine I would not write to signers of the memorial, because I believe that they acted like so many jurymen in a claim against the Government. Nevertheless, if I met any of them or was writing to them on any other subject, I should take the opportunity of expressing my feelings. I think you might with propriety write to Huxley, as he entered so heartily into the scheme and aided in the most important manner in many ways.

Sir J. Lubbock called here yesterday and Mr. F. Balfour came here with one of my sons, and it would have pleased you to see how unfeignedly delighted they were at my news of the success of the memorial.

I wrote also to tell the Duke of Argyll of the success, and he in answer expressed very sincere pleasure.—My dear Wallace,
yours very sincerely,

CH. DARWIN.

Pen-y-bryn, St. Peter's Road, Croydon. January 29, 1881.

My dear Darwin,—Yours just received was very welcome, and the delay in its reaching me is of no importance whatever, as, having seen the announcement of the Queen's approval of the pension, of course I felt it was safe. The antedating of the first payment is a very liberal and thoughtful act; but I do not think it is any way exceptional as regards myself. I am informed it is the custom because, as no payment is made after the death of the person, if the first payment were delayed the proposed recipient might die before the half-year (or quarter-day) and thus receive nothing at all.

I suppose you sent the right address to Mr. Seymour. I have not yet heard from him, but I daresay I shall during the next week.

As I am assured both by Miss Buckley and by Prof. Huxley that it is to you that I owe in the first place this great kindness, and that you have also taken an *immense* amount of trouble to bring it to so successful issue, I must again return you my best thanks, and assure you that there is no one living to whose kindness in such a matter I could feel myself indebted with so much pleasure and satisfaction.—Believe me, dear Darwin, yours very faithfully,

ALFRED R. WALLACE.

Down, Beckenham, Kent. July 9.

My dear Wallace,—Dr. G. Krefft has sent me the enclosed from Sydney. A nurseryman saw a caterpillar feeding on a plant and covered the whole up, but, when he searched for the cocoon [pupa], was long before he could find it, so good was its imitation, in colour and form, of the leaf to which it was attached.

I hope that the world goes well with you. Do not trouble yourself by acknowledging this.—Ever yours,

CH. DARWIN.

Accompanying this letter, which has been published in "Darwin and Modern Science" (1909), was a photograph of the chrysalis (*Papilio sarpedon choredon*) attached to a leaf of its food-plant. Many butterfly pupæ are known to have the power

of individual adjustment to the colours of the particular food-plant or other normal environment; and it is probable that the Australian *Papilio* referred to by Darwin possesses this power.

Nutwood Cottage, Frith Hill, Godalming. July 9, 1881.

My dear Darwin,—I am just doing, what I have rarely if ever done before—reading a book through a second time immediately after the first perusal. I do not think I have ever been so attracted by a book, with perhaps the exception of your “Origin of Species” and Spencer’s “First Principles” and “Social Statics.” I wish therefore to call your attention to it, in case you care about books on social and political subjects, but here there is also an elaborate discussion of Malthus’s “Principles of Population,” to which both you and I have acknowledged ourselves indebted. The present writer, Mr. George, while admitting the main principle as self-evident and as actually operating in the case of animals and plants, denies that it ever has operated or can operate in the case of man, still less that it has any bearing whatever on the vast social and political questions which have been supported by a reference to it. He illustrates and supports his views with a wealth of illustrative facts and a cogency of argument which I have rarely seen equalled, while his style is equal to that of Buckle, and thus his book is delightful reading. The title of the book is “Progress and Poverty.” It has gone through six editions in America, and is now published in England by Kegan Paul. It is devoted mainly to a brilliant discussion and refutation of some of the most widely accepted maxims of political economy, such as the relation of wages and capital, the nature of rent and interest, the laws of distribution, etc., but all treated as parts of the main problem as stated in the title-page, “An Enquiry into the Cause of Industrial Depressions and of Increase of Want with Increase of Wealth.” It is the most startling novel and original book of the last twenty years, and if I mistake not will in the future rank as making an advance in political and social science equal to that made by Adam Smith a century ago.

I am here settled in my little cottage engaged in the occupation I most enjoy—making a garden, and admiring the infinite variety and beauty of vegetable life. I am out of doors all day and hardly read anything. As the long evenings come on I shall get on with

my book on the "Land Question," in which I have found a powerful ally in Mr. George.

Hoping you are well, believe me yours most faithfully,
ALFRED R. WALLACE.

The following is the last letter Wallace received from Darwin, who died on Wednesday, April 19, 1882, in the seventy-fourth year of his age.

Down, Beckenham, Kent. July 12, 1881.

My dear Wallace,—I have been heartily glad to get your note and hear some news of you. I will certainly order "Progress and Poverty," for the subject is a most interesting one. But I read many years ago some books on political economy, and they produced a disastrous effect on my mind, viz. utterly to distrust my own judgment on the subject and to doubt much everyone else's judgment! So I feel pretty sure that Mr. George's book will only make my mind worse confounded than it is at present. I, also, have just finished a book which has interested me greatly, but whether it would interest anyone else I know not: it is "The Creed of Science," by W. Graham, A.M. Who and what he is I know not, but he discusses many great subjects, such as the existence of God, immortality, the moral sense, the progress of society, etc. I think some of his propositions rest on very uncertain foundations, and I could get no clear idea of his notions about God. Notwithstanding this and other blemishes, the book has interested me *extremely*. Perhaps I have been to some extent deluded, as he manifestly ranks too high what I have done.

I am delighted to hear that you spend so much time out of doors and in your garden; for with your wonderful power of observation you will see much which no one else has seen. From Newman's old book (I forget the title) about the country near Godalming, it must be charming.

We have just returned home after spending five weeks on Ullswater: the scenery is quite charming; but I cannot walk, and everything tires me, even seeing scenery, talking with anyone or reading much. What I shall do with my few remaining years of life I can hardly tell. I have everything to make me happy and contented, but life has become very wearisome to me.

I heard lately from Miss Buckley in relation to Lyell's Life, and she mentioned that you were thinking of Switzerland, which I should think and hope you will enjoy much.

I see that you are going to write on the most difficult political question, the Land. Something ought to be done—but what is to rule? I hope that you will [not] turn renegade to natural history; but I suppose that politics are very tempting.

With all good wishes for yourself and family, believe me, my dear Wallace, yours very sincerely,

CHARLES DARWIN.

Wallace's last letter to Darwin was written in October, 1881:

Nutwood Cottage, Frith Hill, Godalming. October 18, 1881.

My dear Darwin,—I have delayed writing to thank you for your book on Worms till I had been able to read it, which I have now done with great pleasure and profit, since it has cleared up many obscure points as to the apparent sinking or burying of objects on the surface and the universal covering up of old buildings. I have hitherto looked upon them chiefly from the gardener's point of view—as a nuisance, but I shall tolerate their presence in the view of their utility and importance. A friend here to whom I am going to lend your book tells me that an agriculturist who had been in West Australia, near Swan River, told him many years ago of the hopelessness of farming there, illustrating the poverty and dryness of the soil by saying, "There are no worms in the ground."

I do not see that you refer to the formation of leaf-mould by the mere decay of leaves, etc. In favourable places many inches or even feet of this is formed—I presume without the agency of worms. If so would it not take part in the formation of all mould? and also the decay of the roots of grasses and of all annual plants, or do you suppose that *all* these are devoured by worms? In reading the book I have not noticed a single erratum.

I enclose you a copy of two letters to the *Mark Lane Express*, written at the request of the editor, and which will show you the direction in which I am now working, and in which I hope to do a little good.—Believe me yours very faithfully,

ALFRED R. WALLACE.

PART III

I.—Wallace's Works on Biology and Geographical Distribution

"I have long recognised how much clearer and deeper your insight into matters is than mine."

"I sometimes marvel how truth progresses, so difficult is it for one man to convince another, unless his mind is vacant."

"I grieve to differ from you, and it actually terrifies me, and makes me constantly distrust myself. I fear we shall never quite understand each other."—DARWIN TO WALLACE.

DURING the period covered by the reception, exposition, and gradual acceptance of the theory of Natural Selection, both Wallace and Darwin were much occupied with closely allied scientific work.

The publication in 1859 of the "Origin of Species"¹ marked a distinct period in the course of Darwin's scientific labours; his previous publications had, in a measure, prepared the way for this, and those which immediately followed were branches growing out from the main line of thought and argument contained in the "Origin," an overflow of the "mass of facts" patiently gathered during the preceding years. With Wallace, the end of the first period of his literary work was completed by the publication of his two large volumes on "The Geographical Distribution of Animals," towards which all his previous thought and writings had tended, and from which, again, came other valuable works leading up to the publication of "Darwinism" (1889).

It will be remembered that Darwin and Wallace, on their respective returns to England, after many years spent in journeyings by land and sea and in laborious research, found the

¹ "It is no doubt the chief work of my life."—C. DARWIN.

first few months fully occupied in going over their large and varied collections, sorting and arranging with scrupulous care the rare specimens they had taken, and in discovering the right men to name and classify them into correct groups.

At this point it will be useful to arrange Darwin's writings under three heads, namely: (1) His zoological and geological books, including "The Voyage of the *Beagle*" (published in 1839), "Coral Reefs" (1842), and "Geological Observations on South America" (1846). In this year he also began his work on *Barnacles*, which was published in 1854; and in addition to the steady work on the "Origin of Species" from 1837 onwards, his observations on "Earthworms," not published until 1881, formed a distinct phase of his study during the whole of these years (1839-59). (2) As a natural sequence we have "Variations of Animals and Plants under Domestication" (1868), "The Descent of Man" (1871), and "The Expression of the Emotions" (1872). (3) What may be termed his botanical works, largely influenced by his evolutionary ideas, which include "The Fertilisation of Orchids" (1862), "Movements and Habits of Climbing Plants" (1875), "Insectivorous Plants" (1876), "The Different Forms of Flowers and Plants of the same Species" (1877), and "The Power of Movement in Plants" (1880).

A different order, equally characteristic, is discovered in Wallace's writings, and it is to be noted that while Darwin devoted himself entirely to scientific subjects, Wallace diverged at intervals from natural science to what may be termed the scientific consideration of social conditions, in addition to his researches into spiritualistic phenomena.

The many enticing interests arising out of the classifying of his birds and insects led Wallace to the conclusion that it would be best to postpone the writing of his book on the Malay Archipelago until he could embody in it the more generally important results derived from the detailed study of certain portions of his collections. Thus it was not until seven years later (1869) that this complete sketch of his travels "from the point of view of the philosophic naturalist" appeared.

Between 1862 and 1867 he wrote a number of articles which were published in various journals and magazines, and he read some important papers before the Linnean, Entomological, and other learned Societies. These included several on physical and

zoological geography; six on questions of anthropology; and five or six dealing with special applications of Natural Selection. As these papers "discussed matters of considerable interest and novelty," such a summary of them may be given as will serve to indicate their value to natural science.

The first of them, read before the Zoological Society in January, 1863, gave some detailed information about his collection of birds brought from Buru. In this he showed that the island was originally one of the Moluccan group, as every bird found there which was not widely distributed was either identical with or closely allied to Moluccan species, while none had special affinities with Celebes. It was clear, then, that this island formed the most westerly outlier of the Mollucan group.

The next paper of importance, read before the same Society in November (1863), was on the birds of the chain of islands extending from Lombok to the great island of Timor. This included a list of 186 species of birds, of which twenty-nine were altogether new. A special feature of the paper was that it enabled him to mark out precisely the boundary line between the Indian and Australian zoological regions, and to trace the derivation of the rather peculiar fauna of these islands, partly from Australia and partly from the Moluccas, but with a strong recent migration of Javanese species due to the very narrow straits separating most of the islands from each other. In "My Life" some interesting tables are given to illustrate how the two streams of immigration entered these islands, and further that "as its geological structure shows . . . Timor is the older island and received immigrants from Australia at a period when, probably, Lombok and Flores had not come into existence or were uninhabitable. . . . We can," he says, "feel confident that Timor has not been connected with Australia, because it has none of the peculiar Australian mammalia, and also because many of the commonest and most widespread groups of Australian birds are entirely wanting."¹

Two other papers, dealing with parrots and pigeons respectively (1864-5), were thought by Wallace himself to be among the most important of his studies of geographical distribution. Writing of them he says: "These peculiarities of distribution

¹"My Life," i. 396-7.

and coloration in two such very diverse groups of birds interested me greatly, and I endeavoured to explain them in accordance with the laws of Natural Selection."

In March, 1864, having begun to make a special study of his collection of butterflies, he prepared a paper for the Linnean Society on "The Malayan Papilionidæ, as illustrating the Theory of Natural Selection." The introductory portion of this paper appeared in the first edition of his volume entitled "Contributions to the Theory of Natural Selection" (1870), but it was omitted in later editions as being too technical for the general reader. From certain remarks found here and there, both in "My Life" and other works, butterflies would appear to have had a special charm and attraction for Wallace. Their varied and gorgeous colourings were a ceaseless delight to his eye, and when describing them one feels the sense of pleasure which this gave him, together with the recollection of the far-off haunts in which he had first discovered them.

This series of papers on birds and insects, with others on the physical geography of the Archipelago and its various races of man, furnished all the necessary materials for the general sketch of the natural history of these islands, and the many problems arising therefrom, which made the "Malay Archipelago" the most popular of his books. In addition to his own personal knowledge, however, some interesting comparisons are drawn between the accounts given by early explorers and the impressions left on his own mind by the same places and people. On the publication of this work, in 1869, extensive and highly appreciative reviews appeared in all the leading papers and journals, and to-day it is still looked upon as one of the most trustworthy and informative books of travel.

When the "Malay Archipelago" was in progress, a lengthy article on "Geological Climates and the Origin of Species" (which formed the foundation for "Island Life" twelve years later) appeared in the *Quarterly Review* (April, 1869). Several references in this to the "Principles of Geology"—Sir Charles Lyell's great work—gave much satisfaction both to Lyell and to Darwin. The underlying argument was a combination of the views held by Sir Charles Lyell and Mr. Croll respectively in relation to the glacial epoch, and the great effect of changed distribution of sea and land, or of differences of altitude, and

how by combining the two a better explanation could be arrived at than by accepting each theory on its own basis.

His next publication of importance was the volume entitled "Contributions to the Theory of Natural Selection," consisting of ten essays (all of which had previously appeared in various periodicals) arranged in the following order:

1. On the Law which has regulated the Introduction of New Species.
2. On the Tendency of Varieties to depart indefinitely from the Original Type.
3. Mimicry, and other Protective Resemblances among Animals.
4. The Malayan Papilionidæ.
5. Instinct in Man and Animals.
6. The Philosophy of Birds' Nests.
7. A Theory of Birds' Nests.
8. Creation by Law.
9. The Development of Human Races under the Law of Natural Selection.
10. The Limits of Natural Selection as applied to Man.

His reasons for publishing this work were, first, that the first two papers of the series had gained him the reputation of being an originator of the theory of Natural Selection, and, secondly, that there were a few important points relating to the origin of life and consciousness and the mental and moral qualities of man and other views on which he entirely differed from Darwin.

Though in later years Wallace's convictions developed considerably with regard to the spiritual aspect of man's nature, he never deviated from the ideas laid down in these essays. Only a very brief outline must suffice to convey some of the most important points.

In the childhood of the human race, he believed, Natural Selection would operate mainly on man's body, but in later periods upon the mind. Hence it would happen that the physical forms of the different races were early fixed in a permanent manner. Sharper claws, stronger muscles, swifter feet and tougher hides determine the survival value of lower animals. With man, however, the finer intellect, the readier adaptability

to environment, the greater susceptibility to improvement, and the elastic capacity for co-ordination, were the qualities which determined his career. Tribes which are weak in these qualities give way and perish before tribes which are strong in them, whatever advantages the former may possess in physical structure. The finest savage has always succumbed before the advance of civilisation. "The Red Indian goes down before the white man, and the New Zealander vanishes in presence of the English settler." Nature, careless in this stage of evolution about the body, selects for survival those varieties of mankind which excel in mental qualities. Hence it has happened that the physical characteristics of the different races, once fixed in very early prehistoric times, have never greatly varied. They have passed out of the range of Natural Selection because they have become comparatively unimportant in the struggle for existence.

After going into considerable detail of organic and physical development, he says: "The inference I would draw from this class of phenomena is, that a superior intelligence has guided the development of man in a definite direction, and for a special purpose, just as man guides the development of many animal and vegetable forms." Thus he foreshadows the conclusion, to be more fully developed in "The World of Life" (1910), of an over-ruling God, of the spiritual nature of man, and of the other world of spiritual beings.

An essay that excited special attention was that on Mimicry. The two on Birds' Nests brought forth some rather heated correspondence from amateur naturalists, to which Wallace replied either by adducing confirmation of the facts stated, or by thanking them for the information they had given him.

With reference to the paper on Mimicry, it is interesting to note that the hypothesis therein adopted was first suggested by H. W. Bates, Wallace's friend and fellow-traveller in South America. The essay under this title dealt with the subject in a most fascinating manner, and was probably the first to arouse widespread interest in this aspect of natural science.

The next eight years saw the production of many important, and valuable works, amongst which the "Geographical Distribution of Animals" (1876) occupies the chief place. This work, though perhaps the least known to the average reader, was considered by Wallace to be the most important scientific work he

ever attempted. From references in letters written during his stay in the Malay Archipelago, it is clear that the subject had a special branch of study and observation many years before he began to work it out systematically in writing. His decision to write the book was the outcome of a suggestion made to him by Prof. A. Newton and Dr. Sclater about 1872. In addition to having already expressed his general views on this subject in various papers and articles, he had, after careful consideration, come to adopt Dr. Sclater's division of the earth's surface into six great zoological regions, which he found equally applicable to birds, mammalia, reptiles, and other great divisions; while at the same time it helped to explain the apparent contradictions in the distribution of land animals. Some years later he wrote:

"In whatever work I have done I have always aimed at systematic arrangement and uniformity of treatment throughout. But here the immense extent of the subject, the overwhelming mass of detail, and above all the excessive diversities in the amount of knowledge of the different classes of animals, rendered it quite impossible to treat all alike. My preliminary studies had already satisfied me that it was quite useless to attempt to found any conclusions on those groups which were comparatively little known, either as regards the proportion of species collected and described, or as regards their systematic classification. It was also clear that as the present distribution of animals is necessarily due to their past distribution, the greatest importance must be given to those groups whose fossil remains in the more recent strata are the most abundant and the best known. These considerations led me to limit my work in its detailed systematic groundwork, and study of the principles and law of distribution, to the mammalia and birds, and to apply the principles thus arrived at to an explanation of the distribution of other groups, such as reptiles, fresh-water fishes, land and fresh-water shells, and the best known insect Orders.

"There remained another fundamental point to consider. Geographical distribution in its practical applications and interest, both to students and to the general reader, consists of two distinct divisions, or rather, perhaps, may be looked at from two points of view. In the first of these we divide the earth

into regions and sub-regions, study the causes which have led to the difference in their animal productions, give a general account of these, with the amount of resemblance to and difference from other regions; and we may also give lists of the families and genera inhabiting each, with indications as to which are peculiar and which are also found in adjacent regions. This aspect of the study I term zoological geography, and it is that which would be of most interest to the resident or travelling naturalist, as it would give him, in the most direct and compact form, an indication of the numbers and kinds of animals he might expect to meet with.”¹

The key-note of the general scheme of distribution, as set forth in these two volumes, may be expressed as an endeavour to compare the extinct and existing fauna of each country and to trace the course by which what is now peculiar to each region had come to assume its present character. The main result being that all the higher forms of life seem to have originally appeared in the northern hemisphere, which has sent out migration after migration to colonise the three southern continents; and although varying considerably from time to time in form and extent, each has kept essentially distinct, while at the same time receiving periodically wave after wave of fresh animal life from the northward.

This again was due to many physical causes such as peninsulas parting from continents as islands, islands joining and making new continents, continents breaking up or effecting junction with or being isolated from one another. Thus Australia received the germ of her present abundant fauna of pouched mammals when she was part of the Old-World continent, but separated from that too soon to receive the various placental mammals which have, except in her isolated area, superseded those older forms. So, also, South America, at one time unconnected with North America, developed her great sloths and armadilloes, and, on fusing with the latter, sent her megatheriums to the north, and received mastodons and large cats in exchange.

Some of the points, such for instance as the division of the sub-regions into which each greater division is separated, gave

¹ “My Life,” ii. 94-5.

rise to considerable controversy. Wallace's final estimate of the work stands: "No one is more aware than myself of the defects of the work, a considerable portion of which are due to the fact that it was written a quarter of a century too soon—at a time when both zoological and palæontological discovery were advancing with great rapidity, while new and improved classifications of some of the great classes and orders were in instant progress. But though many of the details given in these volumes would now require alteration, there is no reason to believe that the great features of the work and general principles established by it will require any important modification."¹

About this time he wrote the article on "Acclimatisation" for the "Encyclopædia Britannica"; and another on "Distribution-Zoology" for the same work. As President of the Biological Section of the British Association he prepared an address for the meeting at Glasgow; wrote a number of articles and reviews, as well as his remarkable book on "Miracles and Modern Spiritualism." In 1878 he published "Tropical Nature," in which he gave a general sketch of the climate, vegetation, and animal life of the equatorial zone of the tropics from his own observations in both hemispheres. The chief novelty was, according to his own opinion, in the chapter on "climate," in which he endeavoured to show the exact causes which produce the difference between the uniform climate of the equatorial zone, and that of June and July in England. Although at that time *we* receive actually more of the light and heat of the sun than does Java or Trinidad in December, yet these places have then a mean temperature very much higher than ours. It contained also a chapter on humming-birds, as illustrating the luxuriance of tropical nature; and others on the colours of animals and of plants, and on various biological problems.²

"Island Life"³ (published 1880) was begun in 1877, and occupied the greater part of the next three years. This had been suggested by certain necessary limitations in the writing of

¹ "My Life," pp. 97-8.

² See "My Life," pp. 98-9.

³ Dr. Henry Forbes in a note to the Editor writes: "In his 'Island Life' Wallace extended his philosophical observations to a wider field, and it is in philosophical biology that Wallace's name must stand pre-eminent for all time." "In our own science of biology," says Profs. Geddes and Thomson in a recent work, "we may recall the 'Grand Old Men,' surely second to none in history—Darwin, Wallace, and Hooker."

“The Geographical Distribution of Animals.” It is a fascinating account of the relations of islands to continents, of their unwritten records of the distribution of plant and animal life in the morning time of the earth, of the causes and results of the glacial period, and of the manner of reckoning the age of the world from geological data. It also included several new features of natural science, and still retains an important place in scientific literature. No better summary can be given than that by the author himself:

“In my ‘Geographical Distribution of Animals’ I had, in the first place, dealt with the larger groups, coming down to families and genera, but taking no account of the various problems raised by the distribution of particular *species*. In the next place, I had taken little account of the various islands of the globe, excepting as forming sub-regions or parts of sub-regions. But I had long seen the great interest and importance of these, and especially of Darwin’s great discovery of the two classes into which they are naturally divided—oceanic and continental islands. I had already given lectures on this subject, and had become aware of the great interest attaching to them, and the great light they threw upon the means of dispersal of animals and plants, as well as upon the past changes, both physical and biological, of the earth’s surface. In the third place, the means of dispersal and colonisation of animals is so connected with, and often dependent on, that of plants, that a consideration of the latter is essential to any broad views as to the distribution of life upon the earth, while they throw unexpected light upon those exceptional means of dispersal which, because they are exceptional, are often of paramount importance in leading to the production of new species and in thus determining the nature of insular floras and faunas.

“Having no knowledge of scientific botany, it needed some courage, or, as some may think, presumption, to deal with this aspect of the problem; but . . . I had long been excessively fond of plants, and . . . interested in their distribution. The subject, too, was easier to deal with, on account of the much more complete knowledge of the detailed distribution of plants than of animals, and also because their classification was in a more advanced and stable condition. Again, some of the most interest-

ing islands of the globe had been carefully studied botanically by such eminent botanists as Sir Joseph Hooker for the Galapagos, New Zealand, Tasmania, and the Antarctic islands; Mr. H. C. Watson for the Azores; Mr. J. G. Baker for Mauritius and other Mascarene islands; while there were floras by competent botanists of the Sandwich Islands, Bermuda and St. Helena. . . .

"But I also found it necessary to deal with a totally distinct branch of science—recent changes of climate as dependent on changes of the earth's surface, including the causes and effects of the glacial epoch, since these were among the most powerful agents in causing the dispersal of all kinds of organisms, and thus bringing about the actual distribution that now prevails. This led me to a careful study of Mr. James Croll's remarkable works on the subject of the astronomical causes of the glacial and interglacial periods. . . . While differing on certain details, I adopted the main features of his theory, combining with it the effects of changes in height and extent of land which form an important adjunct to the meteorological agents. . . .

"Besides this partially new theory of the causes of glacial epochs, the work contained a fuller statement of the various kinds of evidence proving that the great oceanic basins are permanent features of the earth's surface, than had before been given; also a discussion of the mode of estimating the duration of geological periods, and some considerations leading to the conclusion that organic change is now less rapid than the average, and therefore that less time is required for this change than has hitherto been thought necessary. I was also, I believe, the first to point out the great difference between the more ancient continental islands and those of more recent origin, with the interesting conclusions as to geographical changes afforded by both; while the most important novelty is the theory by which I explained the occurrence of northern groups of plants in all parts of the southern hemisphere—a phenomenon which Sir Joseph Hooker had pointed out, but had then no means of explaining."¹

In 1878 Wallace wrote a volume on Australasia for Stanford's "Compendium of Geography and Travel." A later edition was

¹ "My Life," ii. 99-101.

published in 1893, which contained in addition to the physical geography, natural history, and geology of Australia, a much fuller account of the natives of Australia, showing that they are really a primitive type of the great Caucasian family of mankind, and are by no means so low in intellect as had been usually believed. This view has since been widely accepted.

Having, towards the close of 1885, received an invitation from the Lowell Institute, Boston, U.S.A., to deliver a course of lectures in the autumn and winter of 1886, Wallace decided upon a series which would embody those theories of evolution with which he was most familiar, with a special one on "The Darwinian Theory" illustrated by a set of original diagrams on variation. These lectures eventually became merged into the well-known book entitled "Darwinism."

On the first delivery of his lecture on the "Darwinian Theory" at Boston it was no small pleasure to Wallace to find the audience both large and attentive. One of the newspapers expressed the public appreciation in the following truly American fashion: "The first Darwinian, Wallace, did not leave a leg for anti-Darwinism to stand on when he had got through his first Lowell Lecture last evening. It was a masterpiece of condensed statement—as clear and simple as compact—a most beautiful specimen of scientific work. Dr. Wallace, though not an orator, is likely to become a favourite as a lecturer, his manner is so genuinely modest and straightforward."

Wherever he went during his tour of the States this lecture more than all others attracted and pleased his audiences. Many who had the opportunity of conversing with him, and others by correspondence, confessed that they had not been able to understand the "Origin of Species" until they heard the facts explained in such a lucid manner by him. It was this fact, therefore, which led him, on his return home in the autumn of 1887, to begin the preparation of the book ("Darwinism") published in 1889. The method he chose was that of following as closely as possible the lines of thought running through the "Origin of Species," to which he added many new features, in addition to laying special emphasis on the parts which had been most generally misunderstood. Indeed, so fairly and impartially did he set forth the general principles of the Darwinian theory that he was able to say: "Some of my critics declare that I am

more Darwinian than Darwin himself, and in this, I admit, they are not far wrong."

His one object, as set out in the Preface, was to treat the problem of the origin of species from the standpoint reached after nearly thirty years of discussion, with an abundance of new facts and the advocacy of many new and old theories. As it had frequently been considered a weakness on Darwin's part that he based his evidence primarily on experiments with domesticated animals and cultivated plants, Wallace desired to secure a firm foundation for the theory in the variation of organisms in a state of nature. It was in order to make these facts intelligible that he introduced a number of diagrams, just as Darwin was accustomed to appeal to the facts of variation among dogs and pigeons.

Another change which he considered important was that of taking the struggle for existence first, because this is the fundamental phenomenon on which Natural Selection depends. This, too, had a further advantage in that, after discussing variations and the effects of artificial selection, it was possible at once to explain how Natural Selection acts.

The subjects treated with novelty and interest in their important bearings on the theory of Natural Selection were: (1) A proof that all *specific* characters are (or once have been) either useful in themselves or correlated with useful characters (Chap. VI.); (2) a proof that Natural Selection can, in certain cases, increase the sterility of crosses (Chap. VII.); (3) a fuller discussion of the colour relations of animals, with additional facts and arguments on the origin of sexual differences of colour (Chaps. VIII.-X.); (4) an attempted solution of the difficulty presented by the occurrence of both very simple and complex modes of securing the cross-fertilisation of plants (Chap. XI.); (5) some fresh facts and arguments on the wind-carriage of seeds, and its bearing on the wide dispersal of many arctic and alpine plants (Chap. XII.); (6) some new illustrations of the non-heredity of acquired characters, and a proof that the effects of use and disuse, even if inherited, must be overpowered by Natural Selection (Chap. XIV.); and (7) a new argument as to the nature and origin of the moral and intellectual faculties of man (Chap. XV.).

"Although I maintain, and even enforce," wrote Wallace,

“my differences from some of Darwin’s views, my whole work tends forcibly to illustrate the overwhelming importance of Natural Selection over all other agencies in the production of new species. I thus take up Darwin’s earlier position, from which he somewhat receded in the later editions of his works, on account of criticisms and objections which I have endeavoured to show are unsound. Even in rejecting that phase of sexual selection depending on female choice, I insist on the greater efficacy of Natural Selection. This is pre-eminently the Darwinian doctrine, and I therefore claim for my book the position of being the advocate of pure Darwinism.”

In concluding this section which, like a previous one, touches upon the intimate relations between Darwin and Wallace, and the points on which they agreed or differed, it is well, as the differences have been exaggerated and misunderstood, to bear in mind his own declaration: “None of my differences from Darwin imply any real divergence as to the overwhelming importance of the great principle of natural selection, while in several directions I believe that I have extended and strengthened it.”¹

With these explanatory notes the reader will now be able to follow the two groups of letters on Natural Selection, Geographical Distribution, and the Origin of Life and Consciousness which follow.

¹ “My Life,” ii. 22.

PART III.—(*Continued*)

II.—Correspondence on Biology, Geographical Distribution, etc.

[1864-93]

H. SPENCER TO A. R. WALLACE

29 *Bloomsbury Square, W.C.* May 19, 1864.

My dear Sir,—When I thanked you for your little pamphlet¹ the other day, I had not read it. I have since done so with great interest. Its leading idea is, I think, undoubtedly true, and of much importance towards an interpretation of the facts. Though I think that there are some purely physical modifications that may be shown to result from the direct influence of civilisation, yet I think it is quite clear, as you point out, that the small amounts of physical differences that have arisen between the various human races are due to the way in which mental modifications have served in place of physical ones.

I hope you will pursue the inquiry. It is one in which I have a direct interest, since I hope, hereafter, to make use of its results.—Sincerely yours,

HERBERT SPENCER.

SIR C. LYELL TO A. R. WALLACE

53 *Harley Street.* May 22, [1864].

My dear Sir,—I have been reading with great interest your paper on the Origin of the Races of Man, in which I think the question between the two opposite parties is put with such admirable clearness and fairness that that alone is no small

¹ "The Origin of the Races of Man."

assistance towards clearing the way to a true theory. The manner in which you have given Darwin the whole credit of the theory of Natural Selection is very handsome, but if anyone else had done it without allusion to your papers it would have been wrong. . . . With many thanks for your most admirable paper, believe me, my dear Sir, ever very truly yours,

CHA. LYELL.

SIR C. LYELL TO A. R. WALLACE

73 *Harley Street.* March 19, 1867.

Dear Mr. Wallace,—I am citing your two papers in my second volume of the new edition of the "Principles"—that on the Physical Geography of the Malay Archipelago, 1863, and the other on Varieties of Man in ditto, 1864. I am somewhat confounded with the marked line which you draw between the two provinces on each side of the Straits of Lombok. It seems to me that Darwin and Hooker have scarcely given sufficient weight to the objection which it affords to some of their arguments. First, in regard to continental extension, if these straits could form such a barrier, it would seem as if nothing short of a land communication could do much towards fusing together two distinct faunas and floras. But here comes the question—are there any land-quadrupeds in Bali or in Lombok? I think you told me little was known of the plants, but perhaps you know something of the insects. It is impossible that birds of long flight crossing over should not have conveyed the seeds and eggs of some plants, insects, mollusca, etc. Then the currents would not be idle, and during such an eruption as that of Tomboro in Sumbawa all sorts of disturbances, aerial, aquatic and terrestrial, would have scattered animals and plants.

When I first wrote, thirty-five years ago, I attached great importance to preoccupancy, and fancied that a body of indigenous plants already fitted for every available station would prevent an invader, especially from a quite foreign province, from having a chance of making good his settlement in a new country. But Darwin and Hooker contend that continental species which have been improved by a keen and wide competition are most frequently victorious over an insular or more limited flora and fauna. Looking, therefore, upon Bali as an outpost of the great

Old World fauna, it ought to beat Lombok, which only represents a less rich and extensive fauna, namely the Australian.

You may perhaps answer that Lombok is an outpost of an army that may once have been as multitudinous as that of the old continent, but the larger part of the host have been swamped in the Pacific. But they say that European forms of animals and plants run wild in Australia and New Zealand, whereas few of the latter can do the same in Europe. In my map there is a small island called Nousabali; this ought to make the means of migration of seeds and animals less difficult. I cannot find that you say anywhere what is the depth of the sea between the Straits of Lombok, but you mention that it exceeds 100 fathoms. I am quite willing to infer that there is a connection between these soundings and the line of demarcation between the two zoological provinces, but must we suppose land communication for all birds of short flight? Must we unite South America with the Galapagos Islands? Can you refer me to any papers by yourself which might enlighten me and perhaps answer some of these queries? I should have thought that the intercourse even of savage tribes for tens of thousands of years between neighbouring islands would have helped to convey in canoes many animals and plants from one province to another so as to help to confound them. Your hypothesis of the gradual advance of two widely separated continents towards each other seems to be the best that can be offered. You say that a rise of a hundred fathoms would unite the Philippine Islands and Bali to the Indian region. Is there, then, a depth of 600 feet in that narrow strait of Bali, which seems in my map only two miles or so in breadth?

I have [been] confined to the house for a week by a cold or I should have tried to see you. I am afraid to go out to-day.—Believe me ever most truly yours,

CHA. LYELL.

SIR C. LYELL TO A. R. WALLACE

73 Harley Street. April 4, 1867.

My dear Wallace,—I have been reading over again your paper published in 1855 in the *Annals* on "The Law which has regulated the Introduction of New Species"; passages of which

I intend to quote, not in reference to your priority of publication, but simply because there are some points laid down more clearly than I can find in the work of Darwin itself, in regard to the bearing of the geological and zoological evidence on geographical distribution and the origin of species. I have been looking into Darwin's historical sketch thinking to find some allusion to your essay at page xx., 4th ed., when he gets to 1855, but I can find no allusion to it. Yet surely I remember somewhere a passage in which Darwin says in print that you had told him that in 1855 you meant by such expressions as "species being created on the type of pre-existing ones closely allied," and by what you say of modified prototypes, and by the passage in which you ask "what rudimentary organs mean if each species has been created independently," etc., that new species were created by variation and in the way of ordinary generation.

Your last letter was a great help to me, for it was a relief to find that the Lombok barrier was not so complete as to be a source of difficulty. I have also to thank you for your papers, one of which I had read before in the *Natural History Review*, but I am very glad of a separate copy. I am rather perplexed by Darwin speculating on the possibility of New Zealand having once been united with Australia (p. 446, 4th Ed.). The puzzle is greater than I can get over, even looking upon it as an oceanic island. Why should there have been no mammalia, rodents and marsupials, or only one mouse? Even if the Glacial period was such that it was enveloped in a Greenlandic winding-sheet, there would have been some Antarctic animals? It cannot be modern, seeing the height of those alps. It may have been a set of separate smaller islands, an archipelago since united into fewer. No savages could have extirpated mammalia, besides we should have found them fossil in the same places with all those species of extinct *Dinornis* which have come to light. Perhaps you will say that the absence of mammalia in New Caledonia is a corresponding fact.

This reminds me of another difficulty. On the hypothesis of the coral islands being the last remnants of a submerged continent, ought they not to have in them a crowd of peculiar and endemic types, each rivalling St. Helena, instead of which I believe they are very poor [in] peculiar genera. Have they all got submerged for a short time during the ups and downs to which

they have been subjected, Tahiti and some others having been built up by volcanic action in the Pliocene period? Madeira and the Canaries were islands in the Upper Miocene ocean, and may therefore well have peculiar endemic types of very old date, and destroyed elsewhere. I have just got in Wollaston's "Coleoptera Atlantidum," and shall be glad to lend it you when I have read the Introduction. He goes in for continental extension, which only costs him two catastrophes by which the union and disunion with the nearest mainland may readily be accomplished. . . .
—Believe me ever most truly yours, CHA. LYELL.

SIR C. LYELL TO A. R. WALLACE

73 Harley Street. May 2, 1867.

My dear Sir,—I forgot to ask you last night about an ornithological point which I have been discussing with the Duke of Argyll. In Chapter V. of his "Reign of Law" (which I should be happy to lend you, if you have time to look at it immediately) he treats of humming-birds, saying that Gould has made out about 400 species, every one of them very distinct from the other, and only one instance, in Ecuador, of a species which varies in its tail-feathers in such a way as to make it doubtful whether it ought to rank as a species, an opinion to which Gould inclines, or only as a variety or incipient species, as the Duke thinks. For the Duke is willing to go so far towards the transmutation theory as to allow that different humming-birds may have had a common ancestral stock, provided it be admitted that a new and marked variety appears at once with the full distinctness of sex so remarkable in that genus.

According to his notion, the new male variety and the female must both appear at once, and this new race or species must be regarded as an "extraordinary birth." My reason for troubling you is merely to learn, since you have studied the birds of South America, and I hope collected some humming-birds, whether Gould is right in saying that there are so many hundred very distinct species without instances of marked varieties and transitional forms. If this be the case, would it not present us with an exception to the rule laid down by Darwin and Hooker that when a genus is largely represented in a continuous tract of land the species of that genus tend to vary?

I have inquired of Sclater and he tells me that he has a considerable distrust of Gould's information on this point, but that he has not himself studied humming-birds.

In regard to shells, I have always found that dealers have a positive prejudice against intermediate forms, and one of the most philosophical of them, now no more, once confessed to me that it was very much against his trade interest to give an honest opinion that certain varieties were not real species, or that certain forms, made distinct genera by some conchologists, ought not so to rank. Nine-tenths of his customers, if told that it was not a good genus or good species, would say, "Then I need not buy it." What they wanted was names, not things. Of course there are genera in which the species are much better defined than in others, but you would explain this, as Darwin and Hooker do, by the greater length of time during which they have existed, or the greater activity of changes, organic and inorganic, which have taken place in the region inhabited by the generic or family type in question. The manufactory of new species has ceased, or nearly so, and in that case I suppose a variety is more likely to be one of the transitional links which has not yet been extinguished than the first step towards a new permanent race or allied species. . . .

Your last letter will be of great use to me. I had cited the case of beetles recovering from immersion of hours in alcohol from my own experience, but am glad it strikes you in the same light. McAndrew told me last night that the littoral shells of the Azores being European, or rather African, is in favour of a former continental extension, but I suspect that the floating of seaweed containing their eggs may dispense with the hypothesis of the submersion of 1,200 miles of land once intervening. I want naturalists carefully to examine floating seaweed and pumice met with at sea. Tell your correspondents to look out. There should be a microscopic examination of both these means of transport.—Believe me ever truly yours, CHA. LYELL.

SIR C. LYELL TO A. R. WALLACE

73 Harley Street. July 3, 1867.

My dear Wallace,—I was very glad, though I take in the *Westminster Review*, to have a duplicate of your most entertaining

and instructive essay on Mimicry of Colours, etc., which I have been reading with great delight, and I may say that both copies are in full use here. I think it is admirably written and most persuasive.—Believe me ever most truly yours,

CHA. LYELL.

TO HERBERT SPENCER

Hurstpierpoint, Sussex. October 26, 1867.

My dear Mr. Spencer,—After leaving you yesterday I thought a little over your objections to the Duke of Argyll's theory of flight on the ground that it does not apply to insects, and it seems to me that exactly the same general principles do apply to insects as to birds. I read over the Duke's book without paying special attention to that part of it, but as far as I remember, the case of insects offers no difficulty in the way of applying his principles. If any wing were a rigid plane surface, it appears to me that there are only two ways in which it could be made to produce flight. Firstly, on the principle that the resistance in a fluid, and I believe also in air, increases in a greater ratio than the velocity (? as the square), the descending stroke might be more rapid than the ascending one, and the resultant would be an upward or forward motion. Secondly, some kind of furling or feathering by a rotatory motion of the wing might take place on raising the wings. I think, however, it is clear that neither of these actions occurs during the flight of insects. In both slow- and quick-flying species there is no appearance of such a difference of velocity, and I am not aware that anyone has attempted to prove that it occurs; and the fact that in so many insects the edges of the fore and hind wings are connected together, while their insertions at the base are at some distance apart, *entirely precludes a rotation of the wings*. The whole structure and form of the wings of insects, moreover, indicate an action in flight quite analogous to that of birds. I believe that a careful examination will show that the wings of almost all insects are slightly concave beneath. Further, they are all constructed with a strong and rigid anterior margin, while the outer and hinder margins are exceedingly thin and flexible. Yet, further, I feel confident (and a friend here agrees with me) that they are much more rigid against *upward* than against *downward*

pressure. Now in most insects (take a butterfly as an example) the body is weighted behind the insertion of the wings by the long and heavy abdomen, so as to produce an oblique position when freely suspended. There is also much more wing surface behind than before the fulcrum. Now if such an insect produces by muscular action a regular flapping of the wings, flight must result. At the downward stroke the pressure of the air against the hind wings would raise them all to a nearly horizontal position, and at the same time bend up their posterior margins a little, producing an upward and onward motion. At the upward stroke the pressure on the hind wings would depress them considerably into an oblique position, and from their great flexibility in that direction would bend down their hind margins. The resultant would be a slightly downward and considerably onward motion, the two strokes producing that undulating flight, so characteristic of butterflies, and so especially observable in the broad-winged tropical species. Now all this is quite conformable to the action of a bird's wing. The rigid anterior margin, the slender and flexible hind margin; the greater resistance to upward than to downward pressure, and the slight concavity of the under surface, are all characters common to the wings of birds and most insects, and, considering the totally different structure and homologies of the two, I think there is at least an *a priori* case for the function they both subserve being dependent upon these peculiarities. If I remember rightly, it is on these principles that the Duke of Argyll has explained the flight of birds, in which, however, there are of course some specialities depending on the more perfect organisation of the wing, its greater mobility and flexibility, its capacity for enlargement and contraction, and the peculiar construction and arrangement of the feathers. These, however, are matters of detail; and there are no doubt many and important differences of detail in the mode of flight of the different types of insects which would require a special study of each. It appeared to me that the Duke of Argyll had given that special study to the flight of birds, and deserved praise for having done so successfully, although he may not have quite solved the whole problem, or have stated quite accurately the comparative importance of the various causes that combine to effect flight.—

Believe me yours very sincerely,

ALFRED R. WALLACE.

HERBERT SPENCER TO A. R. WALLACE

57 *Queen's Gardens, Bayswater, W.* December 5, 1867.

My dear Wallace,—I did not answer your last letter, being busy in getting out my second edition of "First Principles."

I was quite aware of the alleged additional cause of flight which you name, and do not doubt that it is an aid. But I regard it simply as an aid. If you will move an outstretched wing backwards and forwards with equal velocity, I think you will find that the difference of resistance is nothing like commensurate with the difference in size between the muscles that raise the wings and the muscles that depress them. It seems to me quite out of the question that the principles of flight are fundamentally different in a bat and a bird, which they must be if the Duke of Argyll's interpretation is correct. I write, however, not so much to reply to your argument as to correct a misapprehension which my expressions seem to have given you. The objections are not made by Tyndall or Huxley; but they are objections made by me, which I stated to them, and in which they agreed—Tyndall expressing the opinion that I ought to make them public. I name this because you may otherwise some day startle Tyndall or Huxley by speaking to them of *their* objections, and giving me as the authority for so affiliating them.—Very truly yours,

HERBERT SPENCER.

SIR C. LYELL TO A. R. WALLACE

73 *Harley Street, London, W.* November, 1867.

Dear Wallace,—You probably remember an article by Agassiz in an American periodical, the *Christian Observer*, on the diversity of human races, etc., to prove that each distinct race was originally created for each zoological and botanical province. But while he makes out a good case for the circumscription of the principal races to distinct provinces, he evades in a singular manner the community of the Red Indian race to North and South America. He takes pains to show that the same American race pervades North and South America, or at least all America south of the Arctic region. This was Dr. Morton's opinion, and is, I suppose, not to be gainsaid. In other words, while the Papuan,

Indo-Malayan, Negro and other races are strictly limited each of them to a particular region of mammalia, the Red Indian type is common to Sclater's Neo-arctic and Neo-tropical regions. Have you ever considered the explanation of this fact on Darwinian principles? If there were not barbarous tribes like the Fuegians, one might imagine America to have been peopled when mankind was somewhat more advanced and more capable of diffusing itself over an entire continent. But I cannot well understand why isolation such as accompanies a very low state of social progress did not cause the Neo-tropical and Neo-arctic regions to produce by varieties and Natural Selection two very different human races. May it be owing to the smaller lapse of time, which time, nevertheless, was sufficient to allow of the spread of the representatives of one and the same type from Canada to Cape Horn? Have you ever touched on this subject, or can you refer me to anyone who has?—Believe me ever most truly yours,

CHA. LYELL.

TO SIR C. LYELL

1867.

Dear Sir Charles,—Why the colour of man is sometimes constant over large areas while in other cases it varies, we cannot certainly tell; but we may well suppose it to be due to its being more or less correlated with constitutional characters favourable to life. By far the most common colour of man is a warm brown, not very different from that of the American Indian. White and black are alike deviations from this, and are probably correlated with mental and physical peculiarities which have been favourable to the increase and maintenance of the particular race. I shall infer, therefore, that the brown or red was the original colour of man, and that it maintains itself throughout all climates in America because accidental deviations from it have not been accompanied by any useful constitutional peculiarities. It is Bates's opinion that the Indians are recent immigrants into the tropical plains of South America, and are not yet fully acclimatised.—Yours faithfully,

A. R. WALLACE.

SIR C. LYELL TO A. R. WALLACE

73 Harley Street. March 13, 1869.

Dear Wallace,—. . . I am reading your new book,¹ of which you kindly sent me a copy, with very great pleasure. Nothing equal to it has come out since Darwin's "*Voyage of the Beagle*." . . . The history of the Mias is very well done. I am not yet through the first volume, but my wife is deep in the second and much taken with it. It is so rare to be able to depend on the scientific knowledge and accuracy of those who have so much of the wonderful to relate. . . . —Believe me ever most truly yours,

CHA. LYELL.

CANON KINGSLEY TO A. R. WALLACE

Eversley Rectory, Winchfield. May 5, 1869.

My dear Sir,—I am reading—or rather have all but read—your new book,¹ with a delight which I cannot find words to express save those which are commonplace superlatives. Let me felicitate you on having, at last, added to the knowledge of our planet a chapter which has not its equal (as far as I can recollect) since our friend Darwin's "*Voyage of the Beagle*." Let me, too, compliment you on the modesty and generosity which you have shown, in dedicating your book to Darwin, and speaking of him and his work as you have done. Would that a like unselfish chivalry were more common—I do not say amongst scientific men, for they have it in great abundance, but—in the rest of the community.

May I ask—as a very great favour—to be allowed to call on you some day in London, and to see your insects? I and my daughter are soon, I hope, going to the West Indies, for plants and insects, among other things; and the young lady might learn much of typical forms from one glance at your treasures.

I send this letter by our friend Bates—being ignorant of your address.—Believe me, my dear Sir, ever yours faithfully,

C. KINGSLEY.

¹ "The Malay Archipelago."

TO MISS A. BUCKLEY¹

Holly House, Barking, E. February 2, 1871.

Dear Miss Buckley,—I have read Darwin's first volume,² and like it very much. It is overwhelming as proving the origin of man from some lower form, but that, I rather think, hardly anyone doubts now.

He is very weak, as yet, on my objection about the "hair," but promises a better solution in the second volume.

Have you seen Mivart's book, "Genesis of Species"? It is exceedingly clever, and well worth reading. The arguments against Natural Selection as the exclusive mode of development are some of them exceedingly strong, and very well put, and it is altogether a most readable and interesting book.

Though he uses some weak and bad arguments, and under-rates the power of Natural Selection, yet I think I agree with his conclusion in the main, and am inclined to think it is more philosophical than my own. It is a book that I think will please Sir Charles Lyell.—Believe me yours very truly,

ALFRED R. WALLACE.

TO MISS A. BUCKLEY

Holly House, Barking, E. March 3, 1871.

Dear Miss Buckley,—Thanks for your note. I am hard at work criticising Darwin. I admire his Moral Sense chapter as much as anything in the book. It is both original and the most satisfactory of all the theories, if not quite satisfactory. . . . —Believe me yours very faithfully, ALFRED R. WALLACE.

P.S.—Darwin's book on the whole is wonderful! There are plenty of points open to criticism, but it is a marvellous contribution to the history of the development of the forms of life.

SIR C. LYELL TO A. R. WALLACE

February 15, 1876.

Dear Wallace,—I have read the Preface,³ and like and approve of it much. I do not believe there is a word which Darwin

¹ Private Secretary to Sir Charles Lyell.

² "The Descent of Man."

³ Probably refers to "The Geographical Distribution of Animals,"

would wish altered. It is high time this modest assertion of your claims as an independent originator of Natural Selection should be published.—Ever most truly,
CHA. LYELL.

SIR J. HOOKER TO A. R. WALLACE

Royal Gardens, Kew. August 2, 1880.

My dear Wallace,—I think you have made an immense advance to our knowledge of the ways and means of distribution, and bridged many great gaps.¹ Your reasoning seems to me to be sound throughout, though I am not prepared to receive it in all its details.

I am disposed to regard the Western Australian flora as the latest in point of origin, and I hope to prove it by development, and by the absence of various types. If Western Australia ever had an old flora, I am inclined to suppose that it has been destroyed by the invasion of Eastern types after the union with East Australia. My idea is that these types worked round by the south, and altered rapidly as they proceeded westward, increasing in species. Nor can I conceive the Western Island, when surrounded by sea, harbouring a flora like its present one.

I have been disposed to regard New Caledonia and the New Hebrides as the parent country of many New Zealand and Australian forms of vegetation, but we do not know enough of the vegetation of the former to warrant the conclusion; and after all it would be but a slight modification of your views.

I very much like your whole working of the problem of the isolation and connection of New Zealand and Australia *inter se* and with the countries north of them, and the whole treatment of that respecting north and south migration over the globe is admirable. . . .—Ever most truly yours,
J. D. HOOKER.

SIR J. HOOKER TO A. R. WALLACE

Royal Gardens, Kew. November 10, 1880.

Dear Mr. Wallace,—I have been waiting to thank you for "Island Life" till I should have read it through as carefully as I am digesting the chapters I have finished; but I can delay

¹ The book referred to is Wallace's "Island Life," published in 1880.

no longer, if only to say that I heartily enjoy it, and believe that you have brushed away more cobwebs that have obscured the subject than any other, besides giving a vast deal that is new, and admirably setting forth what is old, so as to throw new light on the whole subject. It is, in short, a first-rate book. I am making notes for you, but hitherto have seen no defect of importance except in the matter of the Bahamas, whose flora is Floridan, not Cuban, in so far as we know it. . . .—Very truly yours,

JOS. D. HOOKER.

TO SIR W. THISELTON-DYER

Pen-y-bryn, St. Peter's Road, Croydon. January 7, 1881.

Dear Mr. Thiselton-Dyer,—If I had had your lecture before me when writing the last chapters of my book I should certainly have quoted you in support of the view of the Northern origin of the Southern flora by migration along existing continents. On reading it again I am surprised to find how often you refer to this; but when I read it on its first appearance I did not pay special attention to this point except to note that your views agreed more closely with those I had advanced, derived from the distribution of animals, than those of any previous writer on botanical distribution. When, at a much later period, on coming to the end of my work, I determined to give a chapter to the New Zealand flora in order to see how far the geological and physical relations between New Zealand and Australia would throw light on its origin, I went for my facts to the works of Sir Joseph Hooker and Mr. Bentham, and also to your article in the “*Encyclopædia Britannica*,” and worked out my conclusions solely from these, and from the few facts referring to the migration of plants which I had collected. Had I referred again to your lecture I should certainly have quoted the cases you give (in a note, p. 431) of plants extending along the Andes from California to Peru and Chile, and vice versa. Whatever identity there is in our views was therefore arrived at independently, and it was an oversight on my part not referring to your views, partly due to your not having made them a more prominent feature of your very interesting and instructive lecture. Working as I do at home, I am obliged to get my facts from the few books

I can get together; and I only attempted to deal with these great botanical questions because the facts seemed sufficiently broad and definite not to be much affected by errors of detail or recent additions to our knowledge, and because the view which I took of the past changes in Australia and New Zealand seemed calculated to throw so much light upon them. Without such splendid summaries of the relations of the Southern floras as are given in Sir J. Hooker's Introductions, I should not have touched the subject at all; and I venture to hope that you or some of your colleagues will give us other such summaries, brought down to the present date, of other important floras—as, for example, those of South Africa and South Temperate America.

Many thanks for additional peculiar British plants. When I hear what Mr. Mitten has to say about the mosses, etc., I should like to send a corrected list to *Nature*, which I shall ask you to be so good as to give a final look over.—Believe me yours very faithfully,

ALFRED R. WALLACE.

P.S.—Mr. Darwin strongly objects to my view of the migration of plants along mountain-ranges, rather than along lowlands during cold periods. This latter view seems to me as difficult and inadequate as mine does to him.—A. R. W.

Wallace was in frequent correspondence with Professor Raphael Meldola, the eminent chemist, a friend both of Darwin and of Wallace, a student of Evolution, and a stout defender of Darwinism. I received from him much help and advice in connection with this work, and had he lived until its completion—he died, suddenly, in 1914—my indebtedness to him would have been even greater.

The following letter to Meldola refers to a suggestion that the white colour of the undersides of animals might have been developed by selection through the *physical* advantage gained from the protection of the vital parts by a *lighter* colour and therefore by a surface of less radiative activity. The idea was that there would be less loss of animal heat through such a white coating. We were at that time unaware of Thayer's demonstration of the value of such colouring for the purposes of con-

cealment among environment. Wallace accepted Thayer's view at once when it was subsequently put forward; as do most naturalists at the present time.

TO PROF. MELDOLA

Frith Hill, Godalming. April 8, 1885.

My dear Meldola,—Your letter in *Nature* last week “riz my dander,” as the Yankees say, and, for once in a way, we find ourselves deadly enemies prepared for mortal combat, armed with steel (pens) and prepared to shed any amount of our own—ink. Consequently I rushed into the fray with a letter to *Nature* intended to show that you are as wrong (as wicked) as are the Russians in Afghanistan. Having, however, the most perfect confidence that the battle will soon be over. . . .—Yours very faithfully,

ALFRED R. WALLACE.

The following letter refers to the theory of physiological selection which had recently been propounded by Romanes, and which Prof. Meldola had criticised in *Nature*, xxxix. 384.

TO PROF. MELDOLA

Frith Hill, Godalming. August 28, 1886.

My dear Meldola,—I have just read your reply to Romanes in *Nature*, and so far as your view goes I agree, but it does not go far enough. Professor Newton has called my attention to a passage in Belt's “Nicaragua,” pp. 207–8, in which he puts forth very clearly exactly your view. I find I had noted the explanation as insufficient, and I hear that in Darwin's copy there is “No! No!” against it. It seems, however, to me to summarise *all* that is of the slightest value in Romanes' wordy paper. I have asked Newton (to whom I had lent it) to forward to you at Birmingham a proof of my paper in the *Fortnightly*, and I shall be much obliged if you will read it carefully, and, if you can, “hold a brief” for me at the British Association in this matter. You will see that a considerable part of my paper is devoted to a demonstration of the fallacy of that part of “Romanes” which declares species to be distinguished generally by

useless characters, and also that "simultaneous variations" do not usually occur.

On the question of sterility, which, as you well observe, is the core of the question, I think I show that it could not work in the way Romanes puts it. The objection to Belt's and your view is, also, that it would not work unless the "sterility variation" was correlated with the "useful variation." You assume, I think, this correlation, when you speak of two of your varieties, B. and K., being *less fertile with the parent form*. Without correlation they could not be so, only some few of them. Romanes always speaks of his physiological variations as being independent, "primary," in which case, as I show, they could hardly ever survive. At the end of my paper I show a correlation which is probably general and sufficient.

In criticising Romanes, however, at the British Association, I want to call your special attention to a point I have hardly made clear enough in my paper. Romanes always speaks of the "physiological variety" as if it were like any other *simple* variety, and could as easily (he says more easily) be increased. Whereas it is really complex, requiring a remarkable correlation between different sets of individuals which he never recognises. To illustrate what I mean, let me suppose a case. Let there occur in a species three individual physiological varieties—A, B and C—each being infertile with the bulk of the species, but quite fertile with some small part of it. Let A, for example, be fertile with X, Y and Z. Now I maintain it to be in the highest degree improbable that B, a quite distinct individual, with distinct parents originating in a distinct locality, and perhaps with a very different constitution, merely because it also is sterile with the bulk of the species, should be fertile with the very same individuals, X, Y, Z, that A is fertile with. It seems to me to be at least 100 to 1 that it will be fertile with some other quite distinct set of individuals. And so with C, and any other similar variety. I express this by saying that each has its "sexual complements," and that the complements of the one are almost sure not to be the complements of the other. Hence it follows that A, B, C, though differing in the same character of general infertility with the bulk of the species, will really be three distinct varieties physiologically, and can in no way unite to form a single physiological variety. This enormous difficulty Ro-

manes apparently never sees, but argues as if all individuals that are infertile with the bulk of the species must be or usually are fertile with the same set of individuals or with each other. This I call a monstrous assumption, for which not a particle of evidence exists. Take this in conjunction with my argument from the severity of the struggle for existence and the extreme improbability of the respective "sexual complements" coming together at the right time, and I think Romanes' ponderous paper is disposed of.

I wrote my paper, however, quite as much to expose the great presumption and ignorance of Romanes in declaring that Natural Selection is *not* a theory of the origin of species—as it is calculated to do much harm. See, for instance, the way the Duke of Argyll jumped at it like a trout at a fly!—Yours very faithfully,

ALFRED R. WALLACE.

The earlier part of the next letter refers to "The Experimental Proof of the Protective Value of Colour and Markings in Insects in reference to their Vertebrate Enemies," in the *Proceedings of the Zoological Society of London*, 1887, p. 191.

TO PROF. POULTON

Frith Hill, Godalming. October 20, 1887.

My dear Poulton,—It is very interesting to me to see how very generally the facts are in accordance with theory, and I am only surprised that the exceptions and irregularities are not more numerous than they are found to be. The only difficult case, that of *D. euphorbiæ*, is due probably to incomplete knowledge. Are lizards and sea-birds the only, or even the chief, possible enemies of the species? They evidently do not prevent its coming to maturity in considerable abundance, and it is therefore no doubt preserved from its chief enemies during its various stages of growth.

The only point on which I differ from you—as you know—is your acceptance, as proved, of the theory of sexual colour selection, and your speaking of insects as having a sense of "the beautiful" in colour, as if that were a known fact. But that is a wide question, requiring full discussion.—Yours very faithfully,

ALFRED R. WALLACE.

TO SIR FRANCIS DARWIN

Frith Hill, Godalming. November 20, 1887.

Dear Mr. Darwin,—Many thanks for the copy of your father's "Life and Letters," which I shall read with very great interest (as will all the world). I was not aware before that your father had been so distressed—or rather disturbed—by my sending him my essay from Ternate, and I am very glad to feel that his exaggerated sense of honour was quite needless so far as I was concerned, and that the incident did not in any way disturb our friendly relations. I always felt, and feel still, that people generally give me far too much credit for my mere sketch of the theory—so very small an affair as compared with the vast foundation of fact and experiment on which your father worked.—Believe me yours very faithfully,

ALFRED R. WALLACE.

TO MRS. FISHER (*née* BUCKLEY)*Frith Hill, Godalming. February 16, 1888.*

My dear Mrs. Fisher,—I know nothing of the physiology of ferns and mosses, but as a matter of fact I think they will be found to increase and diminish together all over the world. Both like moist, equable climates and shade, and are therefore both so abundant in oceanic islands, and in the high regions of the tropics.

I am inclined to think that the reason ferns have persisted so long in competition with flowering plants is the fact that they thrive best in shade, flowers best in the light. In our woods and ravines the flowers are mostly spring flowers, which die away just as the foliage of the trees is coming out and the shade deepens; while ferns are often dormant at that time, but grow as the shade increases.

Why tree-ferns should not grow in cold countries I know not, except that it may be the winds are too violent and would tear all the fronds off before the spores were ripe. Everywhere they grow in ravines, or in forests where they are sheltered, even in the tropics. And they are not generally abundant, but grow in particular zones only. In all the Amazon valley I don't remember ever having seen a tree-fern. . . .

I too am struggling with my "Popular Sketch of Darwinism," and am just now doing a chapter on the great "hybridity" question. I really think I shall be able to arrange the whole subject more intelligibly than Darwin did, and simplify it immensely by leaving out the endless discussion of collateral details and difficulties which in the "Origin of Species" confuse the main issue. . . .

The most remarkable steps yet made in advance are, I think, the theory of Weismann of the continuity of the germ plasm, and its corollary that acquired modifications are never inherited! and Patrick Geddes's explanation of the laws of growth in plants on the theory of the antagonism of vegetative and reproductive growth. . . . —Yours very sincerely,

ALFRED R. WALLACE.

TO PROF. MELDOLA

Frith Hill, Godalming. March 20, 1888.

My dear Meldola,—I have been working away at my hybridity chapters,¹ and am almost disposed to cry "Eureka!" for I have got light on the problem. When almost in despair of making it clear that Natural Selection could act one way or the other, I luckily routed out an old paper that I wrote twenty years ago, giving a demonstration of the action of Natural Selection. It did not convince Darwin then, but it has convinced me now, and I think it can be proved that in some cases (and those I think most probable) Natural Selection will accumulate variations in infertility between incipient species. Many other causes of infertility co-operate, and I really think I have overcome the fundamental difficulties of the question and made it a good deal clearer than Darwin left it. . . . I think also it completely smashes up Romanes.—Yours faithfully,

ALFRED R. WALLACE.

The next letter relates to a question which Prof. Meldola raised as to whether, in view of the extreme importance of "divergence" (in the Darwinian sense) for the separation and maintenance of specific types, it might not be possible that

¹ For the work on "Darwinism."

sterility, when of advantage as a check to crossing, had in itself, as a physiological character, been brought about by Natural Selection, just as extreme fecundity had been brought about (by Natural Selection) in cases where such fecundity was of advantage.

TO PROF. MELDOLA

Frith Hill, Godalming. April 12, 1888.

My dear Meldola,—Many thanks for your criticism. It is a perfectly sound one as against my view being a *complete explanation* of the phenomena, but that I do not claim. And I do not see any chance of the required facts being forthcoming for many years to come. Experiments in the hybridisation of animals are so difficult and tedious that even Darwin never undertook any, and the only people who could and ought to have done it—the Zoological Society—will not. There is one point, however, I think you have overlooked. You urge the improbability of the required infertility being correlated with the particular variations which characterised each incipient species. But the whole point of my argument is, that the physiological adjustments producing fertility are so delicate that they are disturbed by almost any variation or change of conditions—except in the case of domestic animals, which have been domesticated because they are not subject to this disturbance. The whole first half of the chapter is to bring out this fact, which Darwin has dwelt upon, and it certainly does afford a foundation for the assumption that usually, and in some considerable number of individuals, variation in nature, accompanied by somewhat changed conditions of life, is accompanied by, and probably correlated with, some amount of infertility. No doubt this assumption wants proving, but in the meantime I am glad you think that, granting the assumption, I have shown that Natural Selection is able to accumulate sterility variations.

That is certainly a step in advance, and we cannot expect to do more than take very short theoretical steps till we get more facts to rest upon. If you should happen to come across any facts which seem to bear upon it, pray let me know. I can find none but those I have referred to.

I have just finished a chapter on male ornament and display,

which I trust will help to clear up that point.—Believe me yours very faithfully,

ALFRED R. WALLACE.

TO DR. W. B. HEMSLEY

Frith Hill, Godalming. August 26, 1888.

Dear Mr. Hemsley,—You are aware that Patrick Geddes proposes to exclude Natural Selection in the origination of thorns and spines, which he imputes to “diminishing vegetativeness” or “ebbing vitality of the species.” It has occurred to me that insular floras should afford a test of the correctness of this view, since in the absence of mammalia the protection of spines would be less needed.

Your study of these floras will no doubt enable you to answer a few questions on this point. Spines and thorns are, I believe, usually abundant in arid regions of continents, especially in South Africa, where large herbivorous mammals abound. Now, if the long-continued presence of these mammals is a factor in the production of spines by Natural Selection, they should be wholly or comparatively absent in regions equally arid where there are no mammals. The Galapagos seem to be such a case—also perhaps some of the Sandwich Islands, and generally the extra-tropical volcanic islands. Also Australia comparatively, and the highlands of Madagascar.

Of course, the endemic species must be chiefly considered, as they have had time to be modified by the conditions. If you can give me the facts, or your general impression from your study of these floras, I shall be much obliged. I see, of course, many other objections to Geddes's theory, but this seems to offer a crucial test.—Believe me yours very truly,

ALFRED R. WALLACE.

TO DR. W. B. HEMSLEY

Frith Hill, Godalming. September 13, 1888.

Dear Mr. Hemsley,—Many thanks for your interesting letter. The facts you state seem quite to support the usual view, that thorns and spines have been developed as a protection against other animals. The few spiny plants in New Zealand may be

for protection against land-molluscs, of which there are several species as large as any in the tropics. Of course in Australia we should expect only a comparative scarcity of spines, as there are many herbivorous marsupials in the country.—Believe me yours very faithfully,

ALFRED R. WALLACE.

The next and several of the succeeding letters refer to the translations of Weismann's "Essays upon Heredity and Kindred Biological Problems" (Oxford, 1889), and to "Darwinism" (London, 1889).

TO PROF. POULTON

Frith Hill, Godalming. November 4, 1888.

My dear Mr. Poulton,—I returned you the two first of Weismann's essays, with a few notes and corrections in pencil on that on "Duration of Life." Looking over some old papers, I have just come across a short sketch on two pages, on "The Action of Natural Selection in producing Old Age, Decay and Death," written over twenty years ago.¹ I had the same general idea as Weismann, but not that beautiful suggestion of the duration of life, in each case, being the *minimum* necessary for the preservation of the species. *That* I think masterly. The paper on "Heredity" is intensely interesting, and I am waiting anxiously for the concluding part. I will refer to these papers in notes in my book, though perhaps yours will be out first. . . . —Yours faithfully,

A. R. WALLACE.

TO PROF. POULTON

Frith Hill, Godalming. November 8, 1888.

Dear Mr. Poulton,—I return herewith (but separately) the "proofs" I have of Weismann's Essays. The last critical one is rather heavy, and adds nothing of importance to the earlier one on Duration of Life. I enclose my "Note" on the subject, which was written, I think, about 1867, certainly before 1870.

¹ Printed in full as a footnote to Weismann's "Essays upon Heredity," etc.

You will see it was only a few ideas jotted down for further elaboration and then forgotten. I see however it *does* contain the germ of Weismann's argument as to duration of life being determined by the time of securing continuance of the species.—
Yours faithfully, A. R. WALLACE.

TO PROF. POULTON

Frith Hill, Godalming. January 20, 1889.

My dear Mr. Poulton,—My attention has been called by Mr. Herdman, in his Inaugural Address to the Liverpool Biological Society, to Galton's paper on "Heredity," which I read years ago but had forgotten. I have just read it again (in the *Journal of the Anthropological Institute*, Vol. V., p. 329, Jan., 1876), and I find a remarkable anticipation of Weismann's theories which I think should be noticed in a preface to the translation of his book.¹ He argues that it is the undeveloped germs or gemmules of the fertilised ovum that form the sexual elements of the offspring, and thus heredity and atavism are explained. He also argues that, as a corollary, "acquired modifications are barely if at all inherited in the correct sense of the word." He shows the imperfection of the evidence on this point, and admits, just as Weismann does, the heredity of changes in the parent like alcoholism, which, by permeating the whole tissues, may *directly* affect the reproductive elements. In fact, all the main features of Weismann's views seem to be here anticipated, and I think he ought to have the credit of it.

Being no physiologist, his language is not technical, and for this reason, and the place of publication perhaps, his remarkable paper appears to have been overlooked by physiologists.

I think you will find the paper very suggestive, even supplying some points overlooked by Weismann.—Yours faithfully,
A. R. WALLACE.

TO PROF. POULTON

Hamilton House, The Croft, Hastings. February 19, 1889.

Dear Mr. Poulton,—Do you happen to have, or can you easily refer to, Grant Allen's small books of collected papers

¹ See footnote 3, pp. 172-3, of Weismann's "Essays upon Heredity," etc.

under such titles as "Vignettes from Nature," "The Evolutionist at Large," "Colin Clout's Calendar," and another I can't remember? In one of them is a paper on the Origin of Wheat, in which he puts forth the theory that the grasses, etc., are degraded forms which were once insect-fertilised, summing up his views in the phrase, "Wheat is a degraded lily," or something like that. Now Henslow, in his "Floral Structures,"¹ adopts the same theory for all the wind-fertilised or self-fertilised flowers, and he tells me that he is *alone* in the view. I believe the view is a true one, and I want to give G. Allen the credit of first starting it, and want to see how far he went. If you have or can get this work of his with that paper, can you lend it me for a few days? I know not who to write to for it, as botanists of course ignore it, and G. Allen himself is, I believe, in Algeria. . . .
—Yours faithfully, ALFRED R. WALLACE.

HERBERT SPENCER TO A. R. WALLACE

38 *Queen's Gardens, Lancaster Gate, W.* May 18, 1889.

Dear Mr. Wallace,—A few days ago there reached me a copy of your new book, "Darwinism," for which, along with this acknowledgment, I send my thanks. In my present state of health I dare not read, and fear I shall be unable to profit by the accumulation of evidence you have brought together. I see sundry points on which I might raise discussions, but beyond the fact that I am at present unable to enter into them, I doubt whether they would be of any use. I regret that you have used the title "Darwinism," for notwithstanding your qualification of its meaning you will, by using it, tend greatly to confirm the erroneous conception almost universally current.—Truly yours, HERBERT SPENCER.

TO PROF. POULTON

Parkstone, Dorset. November 28, 1889.

My dear Mr. Poulton,—I have much pleasure in sending you Cope's book² (with the review of "Darwinism"), which I hope

¹ "The Origin of Floral Structures through Insect and Other Agencies." Internat. Sci. Series. 1888.

² "The Origin of the Fittest." London, 1887.

you will keep as long as you like, till you have mastered all its obscurities of style and eccentricities of argument. I think you will find a good deal in it to criticise, and it will be well for you to know what the leader of the Neo-Lamarckians regards as the foundation stones of his theory. I greatly enjoyed my visit to Oxford, and only regretted that I could not have more time for personal talk with yourself, for I am so deplorably ignorant of modern physiology that I am delighted to get intelligible explanations of its bearings on the subjects that most interest me in science. I quite see all its importance in investigations of the mechanism of colours, but there is so much still unknown that it will be very hard to convince me that there is no other possible explanation of the peacock's feather than the "continued preference by the females" for the most beautiful males, *in this one point*, "during a long line of descent"—as Darwin says! I expect, however, great light from your new book. . . . —Believe me yours very faithfully,

ALFRED R. WALLACE.

SIR FRANCIS GALTON TO A. R. WALLACE

42 Rutland Gate, S.W. May 24, 1890.

Dear Mr. Wallace,—I send the paper with pleasure, and am glad that you will read it, and I hope then see more clearly than the abstract could show the grounds of my argument.

These finger marks are most remarkable things. Of course I have made out much more about them since writing that memoir. Indeed I have another paper on them next Thursday at the Royal Society, but that only refers to ways of cataloguing them, either for criminal administration, or what I am more interested in, viz. racial and hereditary inquiry.

What I have done in this way is not ready for publication, but I may mention (privately, please) that these persistent marks, which seem fully developed in the sixth month of foetal life, and appear under the reservations and in the evidence published in the memoir to be practically *quite* unchanged during life, are *not* correlated with any ordinary characteristic that I can discover. They are the same in the lowest idiots as in ordinary persons. (I took the impressions of some 80 of these, so idiotic that they mostly could not speak, or even stand, at

the great Darenth Asylum, Dartford.) They are the same in clodhoppers as in the upper classes, and *yet* they are as hereditary as other qualities, I think. Their tendency to symmetrical distribution on the two hands is *marked*, and symmetry *is* a form of kinship. My argument is that sexual selection can have had nothing to do with the patterns, neither can any other form of selection due to vigour, wits, and so forth, because they are not correlated with them. They just go their own gait, uninfluenced by anything that we can find or reasonably believe in, of a *naturally selective influence*, in the plain meaning of the phrase.—Very sincerely yours,

FRANCIS GALTON.

TO THEO. D. A. COCKERELL

Parkstone, Dorset. March 10, 1891.

Dear Mr. Cockerell,— . . . Your theory to account for the influence of a first male on progeny by a second seems very probable—and in fact if, as I suppose, spermatozoa often enter ova without producing complete fertilisation, it must be so. *That* would be easily experimented on, with fowls, dogs, etc., but I do not remember the fact having been observed except with horses. It ought to be common, when females have young by successive males.—Yours faithfully,

A. R. WALLACE.

The next letter relates to a controversy with Romanes concerning Herbert Spencer's argument about Co-adaptation which Romanes had urged in support of Neo-Lamarckism as opposed to Natural Selection. Prof. Meldola endeavoured to show that the difficulties raised by Spencer and supported by Romanes had no real weight because the possibility of so-called "co-adaptations" being developed *successively* in the order of evolution had not been reckoned with. There was no real divergence between Wallace and Prof. Meldola on this matter when they subsequently discussed it. The correspondence is in *Nature*, xliii. 557, and subsequently. See also "Darwin and After Darwin," by Romanes, 1895, ii. 68.

TO PROF. MELDOLA

Parkstone, Dorset. April 25, 1891.

My dear Meldola,—You have now put your foot in it! Romanes *agrees* with you! Henceforth he will claim you as a disciple, converted by his arguments!

There was one admission in your letter I was very sorry to see, because it cannot be strictly true, and is besides open to much misrepresentation. I mean the admission that Romanes pounces upon in his second paragraph. Of course, the number of individuals in a species being finite, the chance of four coincident variations occurring in any one individual—each such variation being separately very common—cannot be anything like “infinity to one.” Why, then, do you concede it most fully?—the result being that Romanes takes you to concede that it is infinity to one against the coincident variations occurring in “*any individuals.*” Surely, with the facts of coincident independent variation we now possess, the occurrence of three, four, or five, coincident variations cannot be otherwise than frequent. As a fact, more than half the whole population of most species seems to vary to a perceptible and measurable, and therefore sufficient, amount in scores of ways. Take a species with a million pairs of individuals—half of these vary sufficiently, either + or —, in the four acquired characters A, B, C, D: what will be the proportion of individuals that vary + in these four characters according to the law of averages? Will it not be about 1 to 64? If so it is ample—in many cases—for Natural Selection to work on, because in many cases less than $\frac{1}{64}$ of offspring survives.

On Romanes’ view of the impossibility of Natural Selection doing anything alone, because the required coincident variations do not occur, the occurrence of a “strong man” or a racehorse that beats all others easily must be impossible, since in each of these cases there must be scores of coincident favourable variations.

Given sufficient variation, I believe divergent modification of a species in two lines could easily occur, even if free intercrossing occurred, because, the numbers varying being a large proportion of the whole, the numbers which bred like with like would be sufficient to carry on the two lines of divergence, those

that intercrossed and produced less perfectly adapted offspring being eliminated. Of course some amount of segregate breeding does always occur, as Darwin always maintained, but, as he also maintained, it is not absolutely essential to evolution. Romanes argues as if "free intercrossing" meant that none would pair like with like! I hope you will have another slap at him, and withdraw or explain that unlucky "infinity to one," which is Romanes' sheet-anchor.—Yours very truly,

ALFRED R. WALLACE.

TO PROF. POULTON

Parkstone, Dorset. June 16, 1892.

My dear Mr. Poulton,—Many thanks for sending me Weismann's additional Essays,¹ which I look forward to reading with much pleasure. I have, however, read the first, and am much disappointed with it. It seems to me the *weakest and most inconclusive* thing he has yet written. At p. 17 he states his theory as to degeneration of eyes, and again, on p. 18, of anthers and filaments; but in both cases he fails to *prove* it, and apparently does not see that his panmixia, or "cessation of selection," cannot possibly produce *continuous* degeneration culminating in the total or almost total disappearance of an organ. Romanes and others have pointed out this weakness in his theory, but he does not notice it, and goes on calmly throughout the essay to *assume* that mere panmixia must cause progressive degeneration to an unlimited extent; whereas all it can do is to effect a reduction to the average of the total population on which selection has been previously worked. He says "individuals with weak eyes would not be eliminated," but omits to notice that individuals with strong eyes would also "not be eliminated," and as there is no reason alleged why variations in *all directions* should not occur as before, the free intercrossing would tend to keep up a mean condition only a little below that which was kept up by selection. It is clear that some form of selection must always co-operate in degeneration, such as economy of growth, which he hardly notices except as a possible but not a necessary factor, or actual injuriousness.

¹ "Essays upon Heredity and Kindred Biological Problems," Vol. II. 1892.

It appears to me that what is wanted is to take a number of typical cases, and in each of them show how Natural Selection comes in to carry on the degeneration begun by panmixia. Weismann's treatment of the subject is merely begging the question.—Yours faithfully,

A. R. WALLACE.

TO PROF. POULTON

Parkstone, Dorset. August 29, 1892.

My dear Mr. Poulton,—As to panmixia you have quite misunderstood my position. By the "mean condition," I do not mean the "mean" during the whole course of development of the organ, as you seem to take it. That would indeed be absurd. I do mean the "mean" of the whole series of individual variations now occurring, during a period sufficient to contain all or almost all the variations to which the species is *now* subject. Take, for instance, such a case as the wings of the swallow, on the full development of which the life of the bird depends. Many individuals no doubt perish for lack of wing-power, due to deficiency in size or form of wing, or in the muscles which move it. The extreme limits of variation would be seen probably if we examined every swallow that had reached maturity during the last century. The average of all those would perhaps be 5 or 10 per cent. below the average of those that survive to become the parents of the next generation in any year; and what I maintain is, that panmixia alone could not reduce a swallow's wings below this first average. Any further reduction must be due either to some form of selection or to "economy of growth"—which is also, fundamentally, a form of selection. So with the eyes of cave animals, panmixia could only cause an imperfection of vision equal to the average of those variations which occurred, say, during a century before the animal entered the cave. It could only produce more effect than this if the effects of disuse are hereditary—which is a non-Weismannian doctrine. I think this is also the position that Romanes took.—Yours faithfully,

A. R. WALLACE.

TO MR. J. W. MARSHALL

Parkstone, Dorset. September 23, 1892.

My dear Marshall,—I am glad you enjoyed Mr. Hudson's book. His observations are inimitable—and his theories and suggestions, if not always the best, at least show thought on what he has observed.

I was most pleased with his demonstration as to the supposed instincts of young birds and lambs, showing clearly that the former at all events are not due to inherited experience, as Darwin thought. The whole book, too, is pervaded by such a true love of nature and such a perception of its marvels and mysteries as to be unique in my experience. The modern scientific morphologists seem so wholly occupied in tracing out the mechanism of organisms that they hardly seem to appreciate the overwhelming marvel of the powers of life, which result in such infinitely varied structures and such strange habits and so-called instincts. The older I grow the more marvellous seem to me the mere variety of form and habit in plants and animals, and the unerring certitude with which from a minute germ the whole complex organism is built up, true to the type of its kind in all the infinitude of details! It is this which gives such a charm to the watching of plants growing, and of kittens so rapidly developing their senses and habitudes! . . . —Yours very faithfully,

ALFRED R. WALLACE.

TO PROF. POULTON

Parkstone, Dorset. February 1, 1893.

My dear Mr. Poulton,—Thanks for the separate copy of your great paper on colours of larva, pupa, etc.¹ I have read your conclusions and looked over some of the experiments, and think you have now pretty well settled that question.

I am reading through the new volume of the *Life of Darwin*, and am struck with the curious example his own case affords of non-heredity of acquired variations. He expresses his constant dread—one of the troubles of his life—that his children

¹ *Trans. Ent. Soc., London, 1892, p. 293.*

would inherit his bad health. It seems pretty clear, from what F. Darwin says in the new edition, that Darwin's constant nervous stomach irritation was caused by his five years seasickness. It was thoroughly established before, and in the early years of, his marriage, and, on his own theory his children ought all to have inherited it. Have they? You know perhaps better than I do, whether any of the family show any symptoms of that particular form of illness—and if not it is a fine case!—Yours very faithfully,

ALFRED R. WALLACE.

Wallace was formally admitted to the Royal Society in June. 1893. The postscript of the following letter refers to his cordial reception by the Fellows.

TO PROF. MELDOLA

Parkstone, Dorset. June 10, 1893.

My dear Meldola,—As we had no time to “discourse” on Thursday, I will say a few words on the individual adaptability question. We have to deal with facts, and facts certainly show that, in many groups, there is a great amount of adaptable change produced in the individual by external conditions, and that that change is not inherited. I do not see that this places Natural Selection in any subordinate position, because this individual adaptability is evidently advantageous to many species, and may itself have been produced or increased by Natural Selection. When a species is subject to great changes of conditions, either locally or at uncertain times, it may be a decided advantage to it to become individually adapted to that change while retaining the power to revert instantly to its original form when the normal conditions return. But whenever the changed conditions are permanent, or are such that individual adaptation cannot meet the requirements, then Natural Selection rapidly brings about a permanent adaptation which is inherited. In plants these two forms of adaptation are well marked and easily tested, and we shall soon have a large body of evidence upon it. In the higher animals I imagine that individual adaptation is small in amount, as indicated by the fact that even slight varieties often breed true.

In Lepidoptera we have the two forms of colour-adaptability clearly shown. Many species are, in all their stages, permanently adapted to their environment. Others have a certain power of individual adaptation, as of the pupæ to their surroundings. If this last adaptation were strictly inherited it would be positively injurious, since the progeny would thereby lose the power of individual adaptability, and thus we should have light pupæ on dark surroundings, and vice versa. Each kind of adaptation has its own sphere, and it is essential that the one should be non-inheritable, the other heritable. The whole thing seems to me quite harmonious and "as it should be."

Thiselton-Dyer tells me that H. Spencer is dreadfully disturbed on the question. He fears that acquired characters may not be inherited, in which case the foundation of his whole philosophy is undermined!—Yours very truly,

ALFRED R. WALLACE.

P.S.—I am afraid you are partly responsible for that kindly meant but too personal manifestation which disturbed the solemnity of the Royal Society meeting on Thursday! . . .

TO PROF. POULTON

Parkstone, Dorset. September 25, 1893.

My dear Poulton,—I suppose you were not at Nottingham and did not get the letter, paper, and photographs I sent you there, but to be opened by the Secretary of Section D in case you were not there. It was about a wonderful and perfectly authenticated case of a woman who dressed the arm of a game-keeper after amputation, and six or seven months afterwards had a child born without the forearm on the right side, exactly corresponding in *form* and *length* of stump to that of the man. Photographs of the man, and of the boy seven or eight years old, were taken *by the physician of the hospital* where the man's arm was cut off, and they show a most striking correspondence. These, with my short paper, appear to have produced an effect, for a committee of Section D has been appointed to collect evidence on this and other matters. . . .—Yours very faithfully,

ALFRED R. WALLACE.

TO PROF. POULTON

Parkstone, Dorset. November 17, 1893.

My dear Poulton,—The letter I wrote to you at Nottingham was returned to me here (after a month) so I did not think it worth while to send it to you again, though it did contain my congratulations on your appointment,¹ which I now repeat. As you have not seen the paper I sent to the British Association, I will just say that I should not have noticed the subject publicly but, after a friend had given me the photographs (sent with my paper), I came across the following statement in the new edition of Chambers' Encyclopædia, art. Deformities (by Prof. A. Hare): "In an increasing proportion of cases which are carefully investigated, it appears that maternal impressions, the result of shock or unpleasant experiences, may have a considerable influence in producing deformities in the offspring." In consequence of this I sent the case which had been furnished me, and which is certainly about as well attested and conclusive as anything can be. The facts are these:

A gamekeeper had his right forearm amputated at the North Devon Infirmary. He left before it was healed, thinking his wife could dress it, but as she was too nervous, a neighbour, a young recently married woman, a farmer's wife, still living, came and dressed it every day till it healed. About six months after she had a child born *without right hand and forearm*, the stump exactly corresponding in length to that of the gamekeeper. Dr. Richard Budd, M.D., F.R.C.P.,² of Barnstaple, the physician to the infirmary, when the boy was five or six years old, himself took a photograph of the boy and the gamekeeper side by side, showing the wonderful correspondence of the two arms. I have these facts *direct from Dr. Budd*, who was personally cognisant of the whole circumstances. A few years after, in November, 1876, Dr. Budd gave an account of the case and exhibited the photographs to a large meeting at the College of Physicians, and I have no doubt it is *one* of the cases referred to in the article I have quoted, though Dr. Budd thinks it has never been published. It will be at once admitted that this is not a chance

¹ As Hope Professor of Zoology in the University of Oxford.

² A member of a family which has produced several eminent medical men.

coincidence, and that all theoretical difficulties must give way to such facts as this. . . . Of course it by no means follows that similar causes should in all cases produce similar effects, since the idiosyncrasy of the mother is no doubt an important factor; but where the combined coincidences are so numerous as in this case—*place, time, person* and exact correspondence of *resulting deformity*—some causal relation must exist.—Believe me yours very truly,

ALFRED R. WALLACE.

PART III.—(Continued)

III.—Correspondence on Biology, Geographical Distribution, etc.

[1894-1913]

HERBERT SPENCER TO A. R. WALLACE

Queen's Hotel, Cliftonville, Margate. August 10, 1894.

Dear Mr. Wallace,—Though we differ on some points we agree on many, and one of the points on which we doubtless agree is the absurdity of Lord Salisbury's representation of the process of Natural Selection based upon the improbability of two varying individuals meeting. His nonsensical representation of the theory ought to be exposed, for it will mislead very many people. I see it is adopted by the *Pall Mall*. I have been myself strongly prompted to take the matter up, but it is evidently your business to do that. Pray write a letter to the *Times* explaining that selection or survival of the fittest does not necessarily take place in the way he describes. You might set out by remarking that whereas he begins by comparing himself to a volunteer colonel reviewing a regiment of regulars, he very quickly changes his attitude and becomes a colonel of regulars reviewing volunteers and making fun of their bunglings. He deserves a severe castigation. There are other points on which his views should be rectified, but this is the essential point.

It behooves you of all men to take up the gauntlet he has thrown down.—Very truly yours, HERBERT SPENCER.

HERBERT SPENCER TO A. R. WALLACE

Queen's Hotel, Cliftonville, Margate. Aug. 19, 1894.

Dear Mr. Wallace,—I cannot at all agree with you respecting the relative importance of the work you are doing and that

which I wanted you to do. Various articles in the papers show that Lord Salisbury's argument is received with triumph, and, unless it is disposed of, it will lead to a public reaction against the doctrine of evolution at large, a far more serious evil than any error which you propose to rectify among biologists. Everybody will look to you for a reply, and if you make no reply it will be understood that Lord Salisbury's objection is valid. As to the non-publication of your letter in the *Times*, that is absurd, considering that your name and that of Darwin are constantly coupled together.—Truly yours,

HERBERT SPENCER.

TO PROF. POULTON

Parkstone, Dorset. September 8, 1894.

My dear Poulton,—I was glad to see your exposure of another American Neo-Lamarckian in *Nature*.¹ It is astonishing how utterly illogical they all are! I was much pleased with your point of the adaptations supposed to be produced by the inorganic environment when they are related to the organic. It is I think new and very forcible. For nearly a month I have been wading through Bateson's book,² and writing a criticism of it, and of Galton who backs him up with his idea of "organic stability." . . . Neither he nor Galton appears to have any adequate conception of what Natural Selection is, or how impossible it is to escape from it. They seem to think that, given a stable variation, and Natural Selection must hide its diminished head!

Bateson's preface, concluding reflections, etc., are often quite amusing. . . . He is so cocksure he has made a great discovery—which is the most palpable of mare's nests.—Yours very truly,

ALFRED R. WALLACE.

P.S.—I allude of course to his grand argument—"environment *continuous*—species *discontinuous*—therefore variations also which produce species must be also *discontinuous*"! (Bateson—q.e.d.).

¹ "Vol. I., p. 445, a review of "A Theory of Development and Heredity," by Henry B. Orr. 1893.

² "Material for the Study of Variation, treated with especial regard to Discontinuity in the Origin of Species." 1894.

TO PROF. POULTON

Parkstone, Dorset. February 19, 1895.

My dear Poulton,—I have read your paper on "Theories of Evolution"¹ with great pleasure. It is very clear and very forcible, and I should think must have opened the eyes of some of your hearers. Your cases against Lamarckism were very strong, and I think quite conclusive. There is one, however, which seems to me weak—that about the claws of lobsters and the tails of lizards moving and acting when detached from the body. It may be argued, fairly, that this is only an incidental result of the extreme muscular irritability and contractibility of the organs, which might have been caused on Lamarckian as well as on the Darwinian hypothesis. The running of a fowl after its head is chopped off is an example of the same kind of thing, and this is certainly not useful. The detachment itself of claw and tail is no doubt useful and adaptive.

When discussing the objection as to failures not being found fossil, there are two additional arguments to those you adduce: (1) Every failure has been, first, a success, or it could not have come into existence (as a species); and (2) The hosts of huge and very specialised animals everywhere recently extinct are clearly failures. They were successes as long as the struggle was with animal competitors only, physical conditions being highly favourable. But, when physical conditions became adverse, as by drought, cold, etc., they failed and became extinct. The entrance of new enemies from another area might equally render them failures. As to your question about myself and Darwin, I had met him once only for a few minutes at the British Museum before I went to the East. . . . —Yours very faithfully,

A. R. WALLACE.

TO MR. CLEMENT REID

Parkstone, Dorset. November 18, 1894.

My dear Clement Reid,— . . . The great, the grand, and long-expected, the prophesied discovery has at last been made—Miocene or Old Pliocene Man in India!!! Good worked

¹ Reprinted in "Essays on Evolution," p. 95. 1908.

flints found *in situ* by the palæontologist to the Geological Survey of India! It is in a ferruginous conglomerate lying beneath 4,000 feet of Pliocene strata and containing hippotherium, etc. But perhaps you have seen the article in *Natural Science* describing it, by Rupert Jones, who, very properly, accepts it! Of course we want the bones, but we have got the flints, and they may follow. Hurrah for the missing link! Excuse more.
—Yours very faithfully,

ALFRED R. WALLACE.

The next letter relates to the rising school of biologists who, in opposition to Darwin's views, held that species might arise by what was at the time termed "discontinuous variation."

TO PROF. MELDOLA

February 4, 1895.

My dear Professor Meldola,—I hope to have copies of my "Evolution" article in a few days, and will send you a couple. The article was in print last September, but, being long, was crowded out month after month, and only now got in by being cut in two. I think I have demolished "discontinuous variation" as having any but the most subordinate part in evolution of species.

Congratulations on Presidency of the Entomological Society.

A. R. WALLACE.

TO PROF. POULTON

Parkstone, Dorset. March 15, 1895.

My dear Poulton,—I have now nearly finished reading Romanes, but do not find it very convincing. There is a large amount of special pleading. On two points only I feel myself hit. My doubt that Darwin really meant that *all* the individuals of a species could be similarly modified without selection is evidently wrong, as he adduces other quotations which I had overlooked. The other point is, that my suggested explanation of sexual ornaments gives away my case as to the utility of all specific characters. It certainly does as it stands, but I now believe, and should have added, that all these orna-

ments, where they differ from species to species, are also recognition characters, and as such were rendered stable by Natural Selection from their first appearance.

I rather doubt the view you state, and which Gulick and Romanes make much of, that a portion of a species, separated from the main body, will have a different average of characters, unless they are a local race which has already been somewhat selected. The large amount of variation, and the regularity of the curve of variation, whenever about 50 or 100 individuals are measured in the same locality, shows that the bulk of a species are similar in amount of variation everywhere. But when a portion of a species begins to be modified in adaptation to new conditions, distinction of some kind is essential, and therefore any slight difference would be increased by selection. I see no reason to believe that species (usually) have been isolated first and modified afterwards, but rather that new species usually arise from species which have a wide range, and in different areas need somewhat different characters and habits. Then *distinctness* arises both by adaptation and by development of recognition marks to minimise intercrossing.

I wonder Darwin did not see that if the unknown "constant causes" he supposes can modify all the individuals of a species, either indifferently, usefully, or hurtfully, and that these characters so produced are, as Romanes says, very, very numerous in all species, and are sometimes the only specific characters, then the Neo-Lamarckians are quite right in putting Natural Selection as a very secondary and subordinate influence, since all it has to do is to weed out the hurtful variations.

Of course, if a species with warning colours were, in part, completely isolated, and its colours or markings were accidentally different from the parent form, whatever set of markings and colours it had would be, I consider, rendered stable for recognition, and also for protection, since if it varied too much the young birds and other enemies would take a heavier toll in learning it was uneatable. It might then be said that the character by which this species differs from the parent species is a useless character. But surely this is not what is usually meant by a "useless character." This is highly useful in itself, though the difference from the other species is not useful. If they were in contact it would be useful, as a distinction preventing inter-

crossing, and so long as they are not brought together we cannot really tell if it is a species at all, since it might breed freely with the parent form and thus return back to one type. The "useless characters" I have always had in mind when arguing this question are those which are or are supposed to be absolutely useless, not merely relatively as regards the difference from an allied species. I think this is an important distinction.—Yours very truly,

ALFRED R. WALLACE.

HERBERT SPENCER TO A. R. WALLACE

64 Avenue Road, Regent's Park, London, N.W.

September 28, 1895.

Dear Mr. Wallace,—As I cannot get you to deal with Lord Salisbury I have decided to do it myself, having been finally exasperated into doing it by this honour paid to his address in France—the presentation of a translation to the French Academy. The impression produced upon some millions of people in England cannot be allowed to be thus further confirmed without protest.

One of the points which I propose to take up is the absurd conception Lord Salisbury sets forth of the process of Natural Selection. When you wrote you said you had dealt with it yourself in your volume on Darwinism. I have no doubt that it is also in some measure dealt with by Darwin himself, by implication or incidentally. You of course know Darwin by heart, and perhaps you would be kind enough to save me the trouble of searching by indicating the relevant passages both in his books and in your own. My reading power is very small, and it tries me to find the parts I want by much reading.—Truly yours,

HERBERT SPENCER.

To the following letter from Mr. Gladstone, Wallace attached this pencil note: "In 1881 I put forth the first idea of mouth-gesture as a factor in the origin of language, in a review of E. B. Tylor's 'Anthropology,' and in 1895 I extended it into an article in the *Fortnightly Review*, and reprinted it with a few further corrections in my 'Studies,' under the title 'The Expressiveness

of Speech or Mouth-Gesture as a Factor in the Origin of Language.' In it I have developed a completely new principle in the theory of the origin of language by showing that every motion of the jaws, lips and tongue, together with inward or outward breathing, and especially the mute or liquid consonants ending words which serve to indicate abrupt or continuous motion, have corresponding meanings in so many cases as to show a fundamental connection. I thus enormously extended the principle of onomatopœia in the origin of vocal language. As I have been unable to find any reference to this important factor in the origin of language, and as no competent writer has pointed out any fallacy in it, I think I am justified in supposing it to be new and important. Mr. Gladstone informed me that there were many thousands of illustrations of my ideas in Homer."—A. R. W.

W. E. GLADSTONE TO A. R. WALLACE

Hawarden Castle, Chester. October 18, 1895.

Dear Sir,—Your kindness in sending me your most interesting article draws on you the inconvenience of an acknowledgment.

My pursuits in connection with Homer, especially, have made me a confident advocate of the doctrine that there is, within limits, a connection in language between sound and sense.

I would consent to take the issue simply on English words beginning with *st*. You go upon a kindred class in *sn*. I do not remember a perfectly *innocent* word, a word habitually used *in bonam partem*, and beginning with *sn*, except the word "snow," and "snow," as I gather from *Schnee*, is one of the worn-down words.

May I beg to illustrate you once more on the ending in *p*. I take our old schoolboy combinations: hop, skip and jump. Each motion an ending motion; and to each word closed with *p* compare the words *run*, *rennen*, *courir*, *currere*.

But I have now a new title to speak. It is deafness; and I know from deafness that I run a worse chance with a man whose mouth is covered with beard and moustache.

A young relation of mine, slightly deaf, was sorely put to it in an University examination because one of his examiners was *secretal* in this way.

I will not trouble you further except to express, with misgiving, a doubt on a single point, the final *f*.

In driving with Lord Granville, who was deaf but not very deaf, I had occasion to mention to him the Duke of *Fife*. I used every effort, but in no way could I contrive to make him hear the word.

I break my word to add one other particular. Out of 27,000 odd lines in Homer, every one of them expressed, in a sense, heavy weight or force; the blows of heavy-armed men on the breastplates of foes . . . [illegible] and the like. — With many thanks, I remain yours very faithfully,

W. E. GLADSTONE.

P.S.—I should say that the efficacy of lip-expression, undeniably, is most subtle, and defies definite description.

TO DR. ARCHDALL REID

Parkstone, Dorset. April 19, 1896.

Dear Sir,—I am sorry I had not space to refer more fully to your interesting work.¹ The most important point on which I think your views require emendation is on *instinct*. I see you quote Spalding's experiments, but these have been quite superseded and shown to be seriously incorrect by Prof. Lloyd Morgan. A paper by him in the *Fortnightly Review* of August, 1893, gives an account of his experiments, and he read a paper on the same subject at the British Association last year. He is now preparing a volume on the subject which will contain the most valuable series of observations yet made on this question. Another point of some importance where I cannot agree with you is your treating dipsomania as a disease, only to be eliminated by drunkenness and its effects. It appears to me to be only a vicious habit or indulgence which would cease to exist in a state of society in which the habit were almost universally reprobated, and the means for its indulgence almost absent. But this is a matter of comparatively small importance.—Believe me yours very truly,

ALFRED R. WALLACE.

¹ "The Present Evolution of Man." 1896.

TO DR. ARCHDALL REID

Parkstone. April 28, 1896.

Dear Sir,—“We can but reason from the facts we know.” We know a good deal of the senses of the higher animals, very little of those of insects. If we find—as I think we do—that all cases of supposed “instinctive knowledge” in the former turn out to be merely intuitive reactions to various kinds of stimulus, combined with very rapidly acquired experience, we shall be justified in thinking that the actions of the latter will some day be similarly explained. When Lloyd Morgan’s book is published we shall have much information on this question. (See “Natural Selection and Tropical Nature,” pp. 91-7.)—
Yours truly, ALFRED R. WALLACE.

TO PROF. MELDOLA

Parkstone, Dorset. October 12, 1896.

My dear Meldola,—I got Weismann’s “Germinal Selection” two or three months back and read it very carefully, and on the whole I admire it very much, and think it does complete the work of ordinary variation and selection. Of course it is a pure hypothesis, and can never perhaps be directly proved, but it seems to me a reasonable one, and it enables us to understand two groups of facts which I have never been able to work out satisfactorily by the old method. These two facts are: (1) the total, or almost total, disappearance of many useless organs, and (2) the continuous development of secondary sexual characters beyond any conceivable utility, and, apparently, till checked by inutility. It explains both these. Disuse alone, as I and many others have always argued, cannot do the first, but can only cause *regression to the mean*, with perhaps some further regression from economy of material.

As to the second, I have always felt the difficulty of accounting for the enormous development of the peacock’s train, the bird of paradise plumes, the long wattle of the bell bird, the enormous tail-feathers of the Guatemalan trogon, of some humming-birds, etc., etc., etc. The beginnings of all these I can explain as recognition marks, and this explains also their distinctive character in allied species, but it does not explain their

growing on and on far beyond what is needful for recognition, and apparently till limited by absolute hurtfulness. It is a relief to me to have "germinal selection" to explain this.

I do not, however, think it at all necessary to explain adaptations, however complex. Variation is so general and so large, in dominant species, and selection is so tremendously powerful, that I believe all needful adaptation may be produced without it. But, if it exists, it would undoubtedly hasten the process of such adaptation and would therefore enable new places in the economy of nature to be more rapidly filled up.

I was thinking of writing a popular exposition of the new theory for *Nature*, but have not yet found time or inclination for it. I began reading "Germinal Selection" with a prejudice against it. That prejudice continued through the first half, but when I came to the idea itself, and after some trouble grasped the meaning and bearing of it, I saw the work it would do and was a convert at once. It really has no relation to Lamarckism, and leaves the non-heredity of acquired characters exactly where it was.—Yours very truly,

ALFRED R. WALLACE.

The next letter relates to the great controversy then being carried on with respect to Weismann's doctrine of the non-inheritance of "acquired" characters, which doctrine implied complete rejection of the last trace of Lamarckism from Darwinian evolution. Wallace ultimately accepted the Weismannian teaching. Darwin had no opportunity during his lifetime of considering this question, which was raised later in an acute form by Weismann.

TO PROF. MELDOLA

Parkstone, Dorset. January 6, 1897.

My dear Meldola,—The passage to which you refer in the "Origin" (top of p. 6) shows Darwin's firm belief in the heredity of acquired variations," and also in the importance of definite variations, that is, "sports," though elsewhere he almost gives these up in favour of indefinite variations; and this last is now the view of all Darwinians, and even of many Lamarckians. I therefore always now assume this as admitted. Weismann's view as to "possible variations" and "impossible varia-

tions" on p. 1 of "Germinal Selection" is misleading, because it can only refer to "sports" or to "cumulative results," not to "individual variations" such as are the material Natural Selection acts on. Variation, as I understand it, can only be a slight modification in the offspring of that which exists in the parent. The question whether pigs could possibly develop wings is absurd, and altogether beside the question, which is, solely, so far as direct evidence goes, as to the means by which the change from one species to another closely allied species has been brought about. Those who want to begin by discussing the causes of change from a dog to a seal, or from a cow to a whale, are not worth arguing with, as they evidently do not comprehend the A, B, C of the theory.

Darwin's ineradicable acceptance of the theory of heredity of the effects of climate, use and disuse, food, etc., on the individual led to much obscurity and fallacy in his arguments, here and there.—Yours very sincerely,

ALFRED R. WALLACE.

TO PROF. POULTON

Parkstone, Dorset. February 14, 1897.

My dear Poulton,—Thanks for copy of your British Association Address,¹ which I did not read in *Nature*, being very busy just then. I have now read it with much pleasure, and think it a very useful and excellent discussion that was much needed. There is, however, one important error, I think, which vitiates a vital part of the argument, and which renders it possible so to reduce the time indicated by geology as to render the accordance of Geology and Physics more easy to effect. The error I allude to was made by Sir A. Geikie in his Presidential Address² which you quote. Immediately it appeared I wrote to him pointing it out, but he merely acknowledged my letter, saying he would consider it. To me it seems a most palpable and extraordinary blunder. The error consists in taking the rate of deposition as the same as the rate of denudation, whereas it is about twenty times as great, perhaps much more—because the

¹ Presidential address in Section D of British Association, 1896, reprinted in "Essays on Evolution," p. 1.

² To the British Association at Edinburgh, 1892.

area of deposition is at least twenty times less than that of denudation. In order to equal the area of denudation, it would require that *every* bed of *every* foundation should have once extended over the *whole* area of all the land of the globe! The deposition in narrow belts along coasts of all the matter brought down by rivers, as proved by the *Challenger*, leads to the same result. In my "Island Life," 2nd Edit., pp. 221-225, I have discussed this whole matter, and on reading it again I can find no fallacy in it. I have, however, I believe, overestimated the time required for deposition, which I believe would be more nearly one-fortieth than one-twentieth that of mean denudation; because there is, I believe, also a great overestimate of the maximum of deposition, because it is partly made up of beds which may have been deposited simultaneously. Also the maximum thickness is probably double the mean thickness.

The mean rate of denudation, both for European rivers and for all the rivers that have been measured, is a foot in three million years, which is the figure that should be taken in calculations.—Believe me yours very truly,

ALFRED R. WALLACE.

TO PROF. MELDOLA

Parkstone, Dorset. April 27, 1897.

My dear Meldola,— . . . I thought Romanes' article in reply to Spencer was very well written and wonderfully clear for him, and I agree with most of it, except his high estimate of Spencer's co-adaptation argument. It is quite true that Spencer's biology rests entirely on Lamarckism, so far as heredity of acquired characters goes. I have been reading Weismann's last book, "The Germ Plasm." It is a wonderful attempt to solve the most complex of all problems, and is almost unreadable without some practical acquaintance with germs and their development.—Believe me yours very faithfully,

ALFRED R. WALLACE.

TO PROF. POULTON

Parkstone, Dorset. June 13, 1897.

My dear Poulton,— . . . The rate of deposition might be modified in an archipelago, but would not necessarily be less than

now, on the *average*. On the ocean side it might be slow, but wherever there were comparatively narrow straits between the islands it might be even faster than now, because the area of deposition would be strictly limited. In the seas between Java and Borneo and between Borneo and Celebes the deposition *may be* above the average. Again, during the development of continents there were evidently extensive mountain ridges and masses with landlocked seas, or inland lakes, and in all these deposition would be rapid. Anyhow, the fact remains that there is no necessary equality between rates of denudation and deposition (in thickness) as Geikie has *assumed*.

I was delighted with your account of Prichard's wonderful anticipation of Galton and Weismann! It is so perfect and complete. . . . It is most remarkable that such a complete statement of the theory and such a thorough appreciation of its effects and bearing should have been so long overlooked. I read Prichard when I was very young, and have never seen the book since. His facts and arguments are really useful ones, and I should think Weismann must be delighted to have such a supporter come from the grave. His view as to the supposed transmission of disease is quite that of Archdall Reid's recent book. He was equally clear as to Selection, and had he been a *zoologist* and *traveller* he might have anticipated the work of both Darwin and Weismann!

To bring out such a book as his "Researches" when only twenty-seven, and a practising physician, shows what a remarkable man he was.—Believe me yours very truly,

ALFRED R. WALLACE.

TO PROF. MELDOLA

Parkstone, Dorset. July 8, 1897.

My dear Meldola,— . . . I am now reading a wonderfully interesting book—O. Fisher's "Physics of the Earth's Crust." It is really a grand book, and, though full of unintelligible mathematics, is so clearly explained and so full of good reasoning on all the aspects of this most difficult question that it is a pleasure to read it. It was especially a pleasure to me because I had just been writing an article on the Permanence of the Oceanic Basins, at the request of the Editor of *Natural Science*, who told

me I was not orthodox on the point. But I find that Fisher supports the same view with very great force, and it strikes me that if weight of argument and number of capable supporters create orthodoxy in science, it is the other side who are not orthodox. I have some fresh arguments, and I was delighted to be able to quote Fisher. It seems almost demonstrated now that Sir W. Thomson was wrong, and that the earth *has* a molten interior and a very thin crust, and in no other way can the phenomena of geology be explained. . . . —Yours very truly,

ALFRED R. WALLACE.

TO SIR OLIVER LODGE

Parkstone, Dorset. March 8, 1898.

My dear Sir,—My own opinion has long been—and I have many times given reasons for it—that there is always an ample amount of variation in all directions to allow any useful modification to be produced, very rapidly, as compared with the rate of those secular changes (climate and geography) which necessitate adaptation; hence no guidance of variation in certain lines is necessary. For proof of this I would ask you to look at the diagrams in Chapter III. of my “Darwinism,” reading the explanation in the text. The proof of such constant indefinite variability has been much increased of late years, and if you consider that instead of tens or hundreds of individuals, Nature has as many thousands or millions to be selected from, every year or two, it will be clear that the materials for adaptation are ample.

Again, I believe that the time, even as limited by Lord Kelvin’s calculations, is ample, for reasons given in Chapter X., “On the Earth’s Age,” in my “Island Life,” and summed up on p. 236. I therefore consider the difficulty set forth on p. 2 of the leaflet you send is not a real one. To my mind, the development of plants and animals from low forms of each is fully explained by the variability proved to exist, with the actual rapid multiplication and Natural Selection. For this no other intellectual agency is required. The problem is to account for the infinitely complex constitution of the material world and its forces which rendered living organisms possible; then, the introduction of consciousness or sensation, which alone rendered

the animal world possible; lastly, the presence in man of capacities and moral ideas and aspirations which could not conceivably be produced by variation and Natural Selection. This is stated at p. 473-8 of my "Darwinism," and is also referred to in the article I enclose (at p. 443) and which you need not return.

The subject is so large and complex that it is not to be wondered so many people still maintain the insufficiency of Natural Selection, without having really mastered the facts. I could not, therefore, answer your question without going into some detail and giving references. . . . —Believe me yours very truly,

ALFRED R. WALLACE.

TO MR. H. N. RIDLEY

Parkstone, Dorset. October 3, 1898.

My dear Mr. Ridley,— . . . We are much interested now about De Rougemont, and I dare say you have seen his story in the *Wide World Magazine*, while in the *Daily Chronicle* there have been letters, interviews and discussions without end. A few people, who think they know everything, treat him as an impostor; but unfortunately they themselves contradict each other, and so far are proved to be wrong more often than De Rougemont. I firmly believe that his story is substantially true—making allowance for his being a foreigner who learnt one system of measures, then lived thirty years among savages, and afterwards had to reproduce all his knowledge in English and Australian idioms. As an intelligent writer in the *Saturday Review* says, putting aside the sensational illustrations there is absolutely nothing in his story but what is quite *possible* and even *probable*. He must have reached Singapore the year after I returned home, and I dare say there are people there who remember Jensen, the owner of the schooner *Veilland*, with whom he sailed on his disastrous pearl-fishing expedition. Jensen is said now to be in British New Guinea, and has often spoken of his lost cargo of pearls. M—— and K—— of the Royal Geographical Society, state that they are convinced of the substantial truth of the main outlines of his story, and after three interviews and innumerable questions are satisfied of his *bona fides*—and so am I.—With best wishes, believe me to be yours very truly,

ALFRED R. WALLACE.

MR. SAMUEL WADDINGTON TO A. R. WALLACE

7 Whitehall Gardens, London, S.W. February 19, 1901.

Dear Sir,—I trust you will forgive a stranger troubling you with a letter, but a friend has asked me whether, as a matter of fact, Darwin held that *all* living creatures descended from one and the same ancestor, and that the pedigree of a humming-bird and that of a hippopotamus would meet if traced far enough back. Can you tell me whether Darwin did teach this?

I should have thought that as life was developed once, it probably could and would be developed many times in different places, as month after month, and year after year went by; and that, from the very first, it probably took many different forms and characters, in the same way as crystals take different forms and shapes, even when composed of the same substance. From these many developments of "life" would descend as many separate lines of evolution, one ending in the humming-bird, another in the hippopotamus, a third in the kangaroo, etc., and their pedigrees (however far back they might be traced) would not join until they reached some primitive form of protoplasm.—Yours faithfully,

SAMUEL WADDINGTON.

TO MR. SAMUEL WADDINGTON

Parkstone, Dorset. February 23, 1901.

Dear Sir,—Darwin believed that all living things originated from "a few forms or from one"—as stated in the last sentence of his "Origin of Species." But privately I am sure he believed in the *one* origin. Of course there is a possibility that there were several distinct origins from inorganic matter, but that is very improbable, because in that case we should expect to find some difference in the earliest forms of the germs of life. But there is no such difference, the primitive germ-cells of man, fish or oyster being almost indistinguishable, formed of identical matter and going through identical primitive changes.

As to the humming-bird and hippopotamus, there is no doubt whatever of a common origin—if evolution is accepted at all; since both are vertebrates—a very high type of organism whose

ancestral forms can be traced back to a simple type much earlier than the common origin of mammals, birds and reptiles.—Yours very truly,

ALFRED R. WALLACE.

TO SIR FRANCIS DARWIN

Parkstone, Dorset. July 3, 1901.

Dear Mr. Darwin,—Thanks for the letter returned. I do hold the opinion expressed in the last sentence of the article you refer to, and have reprinted it in my volume of Studies, etc. But the stress must be laid on the word *proof*. I intended it to enforce the somewhat similar opinion of your father, in the "Origin" (p. 424, 6th Edit.), where he says, "Analogy may be a deceitful guide." But I really do not go so far as he did. For he maintained that there was not any proof that the several great classes or kingdoms were descended from common ancestors.

I maintain, on the contrary, that all without exception are now proved to have originated by "descent with modification," but that there is no proof, and no necessity, that the very same causes which have been sufficient to produce all the species of a genus or Order were those which initiated and developed the greater differences. At the same time I do *not* say they were not sufficient. I merely urge that there is a difference between proof and probability.—Yours very truly,

ALFRED R. WALLACE.

TO PROF. POULTON

Broadstone, Wimborne. August 5, 1904.

My dear Poulton,— . . . What a miserable abortion of a theory is "Mutation," which the Americans now seem to be taking up in place of Lamarckism, "superseded." Anything rather than Darwinism! I am glad Dr. F. A. Dixey shows it up so well in this week's *Nature*,¹ but too mildly!—Yours very truly,

ALFRED R. WALLACE.

¹ Vol. lxx. (1904), p. 313, a review of T. H. Morgan's "Evolution and Adaptation."

TO PROF. POULTON

Broadstone, Wimborne. April 3, 1905.

My dear Poulton,—Many thanks for copy of your Address,¹ which I have read with great pleasure and will forward to Birch next mail. You have, I think, produced a splendid and unanswerable set of facts proving the non-heredity of acquired characters. I was particularly pleased with the portion on “instincts,” in which the argument is especially clear and strong. I am afraid, however, the whole subject is above and beyond the average “entomologist” or insect collector, but it will be of great value to all students of evolution. It is curious how few even of the more acute minds take the trouble to reason out carefully the teaching of certain facts—as in the case of Romanes and the “variable protection,” and as I showed also in the case of Mivart (and also Romanes and Gulick) declaring that isolation alone, without Natural Selection, could produce perfect and well-defined species (see *Nature*, Jan. 12, 1899). . . . —Yours faithfully,

A. R. WALLACE.

TO SIR FRANCIS DARWIN

Broadstone, Wimborne. October 29, 1905.

Dear Mr. Darwin,—I return you the two articles on “Mutation” with many thanks. As they are both supporters of de Vries, I suppose they put his case as strongly as possible. Professor Hubrecht’s paper is by far the clearest and the best written, and he says distinctly that de Vries claims that all new species have been produced by mutations, and none by “fluctuating variations.” Professor Hubrecht supports this and says that de Vries has proved it! And all this founded upon a few “sports” from one species of plant, itself of doubtful origin (variety or hybrid), and offering phenomena in no way different from scores of other cultivated plants. Never, I should think, has such a vast hypothetical structure been erected on so flimsy a basis!

¹ “The Bearing of the Study of Insects upon the Question, Are Acquired Characters Hereditary?” The Presidential Address to the Entomological Society of London, 1905, reprinted in “Essays on Evolution,” p. 139.

The boldness of his statements is amazing, as when he declares (as if it were a fact of observation) that fluctuating variability, though he admits it as the origin of all domestic animals and plants, yet "never leads to the formation of species"! (Hubrecht, p. 216.) There is one point where he so grossly misinterprets your father that I think you or some other botanist should point it out. De Vries is said to quote from "Life and Letters," II., p. 83, where Darwin refers to "chance variations"—explained three lines on as "the slight differences selected by which a race or species is at length formed." Yet de Vries and Hubrecht claim that by "chance variations" Darwin meant "sports" or "mutations," and therefore agrees with de Vries, while both omit to refer to the many passages in which, later, he gave less and less weight to what he termed "single large variations"—the same as de Vries' "mutations"!—Yours very truly,

ALFRED R. WALLACE.

TO SIR JOSEPH HOOKER

Broadstone, Wimborne. November 10, 1905.

My dear Sir Joseph,—I am writing to apologise for a great oversight. When I sent my publishers a list of persons who had contributed to "My Life" in various ways, your name, which should have been *first*, was strangely omitted, and the omission was only recalled to me yesterday by reading your letters to Bates in Clodd's Edition of his Amazon book, which I have just purchased. I now send you a copy by parcel post, in the hope that you will excuse the omission to send it sooner.

Now for a more interesting subject. I was extremely pleased and even greatly surprised, in reading your letters to Bates, to find that at that early period (1862) you were already strongly convinced of three facts which are absolutely essential to a comprehension of the method of organic evolution, but which many writers, even now, almost wholly ignore. They are (1) the universality and large amount of normal variability, (2) the extreme rigour of Natural Selection, and (3) that there is no adequate evidence for, and very much against, the inheritance of acquired characters.

It was only some years later, when I began to write on the

subject and had to think out the exact mode of action of Natural Selection, that I myself arrived at (1) and (2), and have ever since dwelt upon them—in season and out of season, as many will think—as being absolutely essential to a comprehension of organic evolution. The third I did not realise till I read Weismann. I have never seen the sufficiency of normal variability for the modification of species more strongly or better put than in your letters to Bates. Darwin himself never realised it, and consequently played into the hands of the “discontinuous variation” and “mutation” men, by so continually saying “if they vary”—“without variation Natural Selection can do nothing,” etc.

Your argument that variations are not caused by change of environment is equally forcible and convincing. Has anybody answered de Vries yet?

F. Darwin lent me Prof. Hubrecht's review from the *Popular Science Monthly*, in which he claims that de Vries has proved that new species have always been produced from “mutations,” never through normal variability, and that Darwin latterly agreed with him! This is to me amazing! The Americans too accept de Vries as a second Darwin!—Yours very sincerely,

ALFRED R. WALLACE.

SIR J. HOOKER TO A. R. WALLACE

The Camp, Sunningdale. November 12, 1905.

My dear Wallace,—My return from a short holiday at Sidmouth last Thursday was greeted by your kind and welcome letter and copy of your “Life.” The latter was, I assure you, never expected, knowing as I do the demand for free copies that such a work inflicts on the writer. In fact I had put it down as one of the annual Christmas gifts of books that I receive from my own family. Coming, as it thus did, quite unexpectedly, it is doubly welcome, and I do heartily thank you for this proof of your greatly valued friendship. It will prove to be one of four works of greatest interest to me of any published since Darwin's “Origin,” the others being Waddell's “Lhasa,” Scott's “Antarctic Voyage,” and Mill's “Siege of the South Pole.”

I have not seen Clodd's edition of Bates's “Amazon,” which

I have put down as to be got, and I had no idea that I should have appeared in it. Your citation of my letters and their contents are like dreams to me; but to tell you the truth, I am getting dull of memory as well as of hearing, and what is worse, in reading: what goes in at one eye goes out at the other. So I am getting to realise Darwin's consolation of old age, that it absolves me from being expected to know, remember, or reason upon new facts and discoveries. And this must apply to your query as to anyone having as yet answered de Vries. I cannot remember having seen any answer; only criticisms of a discontinuous sort. I cannot for a moment entertain the idea that Darwin ever assented to the proposition that new species have always been produced from mutation and never through normal variability. Possibly there is some quibble on the definition of mutation or of variation. The Americans are prone to believe any new things, witness their swallowing the thornless cactus produced by that man in California—I forget his name—which Kew exposed by asking for specimens to exhibit in the Cactus House. . . . —I am, my dear Wallace, sincerely yours,

JOS. D. HOOKER.

TO MR. E. SMEDLEY

Broadstone, Wimborne. January 31, 1906.

Dear Mr. Smedley,—I have read Oliver Lodge's book in answer to Haeckel, but I do not think it very well done or at all clearly written or well argued. A book ¹ has been sent me, however, which is a masterpiece of clearness and sound reasoning on such difficult questions, and is a far more crushing reply to Haeckel than O. Lodge's. I therefore send you a copy, and feel sure you will enjoy it. It is a stiff piece of reasoning, and wants close attention and careful thought, but I think you will be able to appreciate it. In my opinion it comes as near to an intelligible solution of these great problems of the Universe as we are likely to get while on earth. It is a book to read and think over, and read again. It is a masterpiece. . . . —Yours very truly,

ALFRED R. WALLACE.

¹ Probably "Root Principles," by Child.

TO PROF. POULTON

Broadstone, Wimborne. July 27, 1907.

My dear Poulton,—Thanks for your very interesting letter. I am glad to hear you have a new book on “Evolution”¹ nearly ready and that in it you will do something to expose the fallacies of the Mutationists and Mendelians, who pose before the world as having got *all* wisdom, before which we poor Darwinians must hide our diminished heads!

Wishing to know the best that could be said for these latter-day anti-Darwinians, I have just been reading Lock’s book on “Variation, Heredity, and Evolution.” In the early part of his book he gives a tolerably fair account of Natural Selection, etc. But he gradually turns to Mendelism as the “one thing needful” —stating that there can be “no sort of doubt” that Mendel’s paper is the “most important” contribution of its size ever made to biological science!

“Mutation,” as a theory, is absolutely nothing new—only the assertion that new species originate *always* in sports, for which the evidence adduced is the most meagre and inconclusive of any ever set forth with such pretentious claims! I hope you will thoroughly expose this absurd claim.

Mendelism is something new, and within its very limited range, important, as leading to conceptions as to the causes and laws of heredity, but only misleading when adduced as the true origin of species in nature, as to which it seems to me to have no part. —Yours very truly,

ALFRED R. WALLACE.

TO PROF. POULTON

Broadstone, Wimborne. November 26, 1907.

My dear Poulton,—Many thanks for letting me see the proofs.² . . . The whole reads very clearly, and I am delighted with the way you expose the Mendelian and Mutational absurd claims. That ought to really open the eyes of the newspaper men to the fact that Natural Selection and Darwinism are not *only* holding their ground but are becoming more firmly established than

¹ “Essays on Evolution.” 1908.

² Of the Introduction to “Essays on Evolution.”

ever by every fresh research into the ways and workings of living nature. I shall look forward to great pleasure in reading the whole book. I was greatly pleased with Archdall Reid's view of Mendelism in *Nature*.¹ He is a very clear and original thinker.

I see in Essay X. you use in the title the term "defensive coloration." Why this instead of the usual "protective"? Surely the whole function of such colours and markings is to protect from attack—not to defend when attacked. The latter is the function of stings, spines and hard coats. I only mention this because using different terms may lead to some misconception.

Your illustration of mutation by throwing colours on a screen, and the argument founded on it, I liked much. That reminds me that H. Spencer's argument for inheritance of acquired variations—that co-ordination of many parts at once, required for adaptations, would be impossible by chance variations of those parts—applies with a hundredfold force to mutations, which are admittedly so much less frequent both in their numbers and the repetitions of them.—Yours very truly,

ALFRED R. WALLACE.

TO PROF. POULTON

Broadstone, Wimborne. December 18, 1907.

My dear Poulton,—The importance of Mendelism to Evolution seems to me to be something of the same kind, but very much less in degree and importance, as Galton's fine discovery of the law of the average share each parent has in the characters of the child—one quarter, the four grandparents each one-sixteenth, and so on. That illuminates the whole problem of heredity, combined with individual diversity, in a way nothing else does. I almost wish you could introduce that!—Yours very truly,

ALFRED R. WALLACE.

TO DR. ARCHDALL REID

Broadstone, Wimborne. January 19, 1908.

Dear Sir,— . . . I was much pleased the other day to read, in a review of Mr. T. Rice Holmes's fine work on "Ancient Britain

¹ Vol. lxxvii., p. 54, a note "On the Interpretation of Mendelian Phenomena."

and the Invasions of Julius Cæsar," that the author has arrived by purely historical study at the conclusion that we have not risen morally above our primitive ancestors. It is a curious and important coincidence.

I myself got the germ of the idea many years ago, from a very acute thinker, Mr. Albert Mott, who gave some very original and thoughtful addresses as President of the Liverpool Philosophical Society, one of which dealt with the question of savages being often, perhaps always, the descendants of more civilised races, and therefore affording no proof of progression. At that time (about 1860-70) I could not accept the view, but I have now come to think he was right.—Yours very truly,

ALFRED R. WALLACE.

TO PROF. POULTON

Old Orchard, Broadstone, Wimborne. November 2, 1908.

My dear Poulton,— . . . You may perhaps have heard that I have been invited by the Royal Institution (through Sir W. Crookes) to give them a lecture on the jubilee of the "Origin of Species" in January. After some consideration I accepted, because I *think* I can give a broad and general view of Darwinism, that will finally squash up the Mutationists and Mendelians, and be both generally intelligible and interesting. So far as I know this has never yet been done, and the Royal Institution audience is just the intelligent and non-specialist one I shall be glad to give it to if I can.

I have been very poorly the last three weeks, but am now recovering my health and strength slowly. It will take me all my time the next two months to get this ready, and now I must write a letter in reply to the absurd and gross misrepresentation of Prof. Hubrecht, as to imaginary differences between Darwin and myself, in the last *Contemporary*!—Yours very truly,

ALFRED R. WALLACE.

The next letter relates to Wallace's Friday evening Discourse at the Royal Institution. His friends were afraid whether his voice could be sustained throughout the hour—fears which were abundantly dispelled by the actual performance. This was his last public lecture.

TO PROF. MELDOLA

Old Orchard, Broadstone, Wimborne. December 20, 1908.

My dear Meldola,—Thanks for your kind offer to read for me if necessary. But when Sir Wm. Crookes first wrote to me about it, he offered to read all, or any parts of the lecture, if my voice did not hold out. I am very much afraid I cannot stand the strain of speaking beyond my natural tone for an hour, or even for half that time—but I may be able to do the opening and conclusion. . . .

I am glad that you see, as I do, the utter futility of the claims of the Mutationists. I may just mention them in the lecture, but I hope I have put the subject in such a way that even “the meanest capacity” will suffice to see the absurdity of their claims.—Yours very truly,

ALFRED R. WALLACE.

TO PROF. POULTON

Old Orchard, Broadstone, Wimborne. January 26, 1909.

My dear Poulton,—I had a delightful two hours at the Museum on Saturday morning, as Mr. Rothschild brought from Tring several of his glass-bottomed drawers with his finest new New Guinea butterflies. They *were* a treat! I never saw anything more lovely and interesting! . . .

As to your very kind and pressing invitation,¹ I am sorry to be obliged to decline it. I cannot remain more than one day or night away from home, without considerable discomfort, and all the attractions of your celebration are, to me, repulsions. . . .

My lecture, even as it will be published in the *Fortnightly*, will be far too short for exposition of all the points I wish to discuss, and I hope to occupy myself during this year in saying all I want to say in a book (of a wider scope) which is already arranged for. One of the great points, which I just touched on in the lecture, is to show that all that is usually considered the waste of Nature—the enormous number produced in proportion to the few that survive—was absolutely essential in order to

¹ The Oxford Celebration of the Hundredth Anniversary of the Birth of Charles Darwin, February 12, 1809. An account of the celebration is given in “Darwin and ‘The Origin,’” by E. B. Poulton, p. 78. 1909.

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secure the variety and continuity of life through all the ages, and especially of that one line of descent which culminated in man. That, I think, is a subject no one has yet dealt with.—Yours very faithfully,

ALFRED R. WALLACE.

TO PROF. POULTON

Old Orchard, Broadstone, Wimborne. March 1, 1909.

Dear Poulton,— . . . I am glad that Lankester has replied to the almost disgraceful Centenary article in the *Times*. But it is an illustration of the widespread mischief the Mutationists, etc., are doing. I have no doubt, however, it will all come right in the end, though the end may be far off, and in the meantime we must simply go on, and show, at every opportunity, that Darwinism actually does explain the whole fields of phenomena that they do not even attempt to deal with, or even approach. . . .—Yours very truly,

ALFRED R. WALLACE.

TO MRS. FISHER

Old Orchard, Broadstone, Wimborne. March 6, 1909.

Dear Mrs. Fisher,— . . . Another point I am becoming more and more impressed with is, a teleology of fundamental laws and forces rendering development of the infinity of life-forms possible (and certain) in place of the old teleology applied to the production of each species. Such are the case of feathers reproduced annually, which I gave at end of lecture, and the still more marvellous fact of the caterpillar, often in two or three weeks of chrysalis life, having its whole internal, muscular, nervous, locomotive and alimentary organs decomposed and re-composed into a totally different being—an absolute miracle if ever there is one, quite as wonderful as would be the production of a complex marine organism out of a mass of protoplasm. Yet because there has been continuity, the difficulty is slurred over or thought to be explained!—Yours very truly,

ALFRED R. WALLACE.

TO SIR W. T. THISELTON-DYER

Old Orchard, Broadstone, Wimborne. June 22, 1909.

Dear Sir William,—On Saturday, to my great pleasure, I received a copy of the Darwin Commemoration volume. I at once began reading your most excellent paper on the Geographical Distribution of Plants. It is intensely interesting to me, both because it so clearly brings out Darwin's views and so judiciously expounds his arguments—even when you intimate a difference of opinion—but especially because you bring out so clearly and strongly his views on the general permanence of continents and oceans, which to-day, as much as ever, wants insisting upon. I may just mention here that none of the people who still insist on former continents where now are deep oceans have ever dealt with the almost physical impossibility of such a change having occurred without breaking the continuity of terrestrial life, owing to the mean depth of the ocean being at least six times the mean height of the land, and its area nearly three times, so that the whole mass of the land of the existing continents would be required to build up even *one small* continent in the depths of the Atlantic or Pacific! I have demonstrated this, with a diagram, in my "Darwinism" (Chap. XII.), and it has never been either refuted or noticed, but passed by as if it did not exist! Your whole discussion of Dispersal and Distribution is also admirable, and I was much interested with your quotation from Guppy, whose book I have not seen, but must read.

Most valuable to me also are your numerous references to Darwin's letters, so that the article serves as a compendious index to the five volumes, as regards this subject.

Especially admirable is the way in which you have always kept Darwin before us as the centre of the whole discussion, while at the same time fairly stating the sometimes adverse views of those who differ from him on certain points. . . . —
Yours very truly, ALFRED R. WALLACE.

SIR W. T. THISELTON-DYER TO A. R. WALLACE

The Ferns, Witcombe, Gloucester. June 25, 1909.

Dear Dr. Wallace,—It is difficult for me to tell you how gratified I am by your extraordinarily kind letter. . . . The truth

is that success was easy. It has been my immense good fortune to know most of those who played in the drama. The story simply wanted a straightforward amanuensis to tell itself. But it is a real pleasure to me to know that I have met with some measure of success.

There are many essays in the book that you will not like any more than I do. The secret of this lies in the fact, which you¹ pointed out in your memorable speech at the Linnean Celebration, that no one but a naturalist can really understand Darwin.

I did not go to Cambridge—I had my hands full here. I was not sorry for the excuse. There seemed to me a note of insincerity about the whole business. I am short-tempered. I cannot stand being told that the origin of species has still to be discovered, and that specific differences have no “reality” (Bateson’s Essay, p. 89). People are of course at liberty to hold such opinions, but decency might have presented another occasion for ventilating them.—Yours sincerely,

W. T. THISELTON-DYER.

SIR W. T. THISELTON-DYER TO A. R. WALLACE

The Ferns, Witcombe, Gloucester. July 11, 1909.

Dear Mr. Wallace,— . . . I have just got F. Darwin’s “Foundations.” He tries to make out that his father could have dispensed with Malthus. But the selection death-rate in a slightly varying large population is *the* pith of the whole business. The Darwin-Wallace theory is, as you say, “the continuous adjustment of the organic to the inorganic world.” It is what mathematicians call “a moving equilibrium.” In fact, I have always maintained that it is a mathematical conception.

It seemed to me there was a touch of insincerity about the whole celebration,¹ as the younger Cambridge School as a whole do not even begin to understand the theory. . . . I take it that the reason is, as you pointed out, that none of them are Naturalists.—Yours sincerely,

W. T. THISELTON-DYER.

¹ The Darwin Celebration.

TO DR. ARCHDALL REID

Old Orchard, Broadstone, Dorset. December 28, 1909.

Dear Dr. Archdall Reid,—Many thanks for your very interesting and complimentary letter. I am very glad to hear of your new book, which I doubt not will be very interesting and instructive. The subjects you treat are, however, so very complex, and require so much accurate knowledge of the facts, and so much sound reasoning upon them, that I cannot possibly undertake the labour and thought required before I should feel justified in expressing an opinion upon your treatment of them. . . .

I rejoice to hear that you have exposed the fallacy of the claims of the Mendelians. I have also tried to do so, but I find it quite impossible for me to follow their detailed studies and arguments. It wants a mathematical mind, which I have not.

But on the general relation of Mendelism to Evolution I have come to a very definite conclusion. This is, that it has no relation whatever to the evolution of species or higher groups, but is really antagonistic to such evolution! The essential basis of evolution, involving as it does the most minute and all-pervading adaptation to the whole environment, is extreme and ever-present plasticity, as a condition of survival and adaptation. But the essence of Mendelian characters is their rigidity. They are transmitted without variation, and therefore, except by the rarest of accidents, can never become adapted to ever varying conditions. Moreover, when crossed they reproduce the same pair of types in the same proportions as at first, and therefore without selection; they are antagonistic to evolution by continually reproducing injurious or useless characters—which is the reason they are so rarely found in nature, but are mostly artificial breeds or sports. My view is, therefore, that Mendelian characters are of the nature of abnormalities or monstrosities, and that the “Mendelian laws” serve the purpose of eliminating them when, as usually, they are not useful, and thus preventing them from interfering with the normal process of natural selection and adaptation of the more plastic races. I am also glad to hear of your new argument for non-inheritance of acquired characters.—Yours very truly,

ALFRED R. WALLACE.

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TO SIR W. T. THISELTON-DYER

Old Orchard, Broadstone, Wimborne. February 8, 1911.

Dear Sir W. Thiselton-Dyer,—I thank you very much for taking so much trouble as you have done in writing your views of my new book.¹ I am glad to find that you agree with much of what I have said in the more evolutionary part of it, and that you differ only on some of my suggested interpretations of the facts. I have always felt the disadvantage I have been under—more especially during the last twenty years—in having not a single good biologist anywhere near me, with whom I could discuss matters of theory or obtain information as to matters of fact. I am therefore the more pleased that you do not seem to have come across any serious misstatements in the botanical portions, as to which I have had to trust entirely to second-hand information, often obtained through a long and varied correspondence.

As to your disagreement from me in the conclusions arrived at and strenuously advocated in the latter portions of my work, I am not surprised. I am afraid, now, that I have not expressed myself sufficiently clearly as to the fundamental phenomena which seem to me absolutely to necessitate a guiding mind and organising power. Hardly one of my critics (I think absolutely not one) has noticed the distinction I have tried and intended to draw between Evolution on the one hand, and the fundamental powers and properties of Life—growth, assimilation, reproduction, heredity, etc.—on the other. In Evolution I recognise the action of Natural Selection as universal and capable of explaining all the facts of the continuous development of species from species, “from amoeba to man.” But this, as Darwin, Weismann, Kerner, Lloyd-Morgan, and even Huxley have seen, has nothing whatever to do with the basic mysteries of life—growth, etc., etc. The chemists think they have done wonders when they have produced in their laboratories certain organic substances—always by the use of other organic products—which life builds up within each organism, and from the few simple elements available in air, earth, and water, innumerable struc-

¹ “The World of Life.”

tures—bone, horn, hair, skin, blood, muscle, etc., etc.; and these are not amorphous—mere lumps of dead matter—but organised to serve certain definite purposes in each living organism. I have dwelt on this in my chapter on “The Mystery of the Cell.” Now I have been unable to find any attempt by any biologist or physiologist to grapple with this problem. One and all, they shirk it, or simply state it to be insoluble. It is here that I state guidance and organising power are essential. My little physiological parable or allegory (p. 296) I think sets forth the difficulty fairly, though by no means adequately, yet not one of about fifty reviews I have read even mentions it.

If you know of any writer of sufficient knowledge and mental power, who has fully recognised and fairly grappled with this fundamental problem, I should be very glad to be referred to him. I have been able to find no approach to it. Yet I am at once howled at, or sneered at, for pointing out the facts that such problems exist, that they are not in any way touched by Evolution, but are far before it, and the forces, laws and agencies involved are those of existences possessed of powers, mental and physical, far beyond those mere mechanical, physical, or chemical forces we see at work in nature. . . . —Yours very truly,

ALFRED R. WALLACE.

SIR W. T. THISELTON-DYER TO A. R. WALLACE

The Ferns, Witcombe, Gloucester. February 12, 1911.

Dear Mr. Wallace,— . . . You must let me correct you on one technical point in your letter. It is no longer possible to say that chemists effect the synthesis of organic products “by the use of other organic substances.” From what has been already effected, it cannot be doubted that eventually every organic substance will be built up from “the few simple elements available in air, earth and water.” I think you may take it from me that this does not admit of dispute. . . .

At any rate we are in agreement as to Natural Selection being capable of explaining evolution “from amoeba to man.”

It is generally admitted that that is a mechanical or scientific explanation. That is to say, it invokes nothing but intelligible actions and causes.

De Vries, however, asserts that the Darwinian theory is *not*

scientific at all, and that is of course a position he has a right to take up.

But if we admit that it is scientific, then we are precluded from admitting a "directive power."

This was von Baer's position, also that of Kant and of Weismann.

But von Baer remarks that the naturalist is not precluded from asking "whether the *totality* of details leads him to a general and final basis of intentional design." I have no objection to this, and offer it as an olive-branch which you can throw to your howling and sneering critics.

As to "structures organised to serve certain definite purposes," surely they offer no more difficulty as regards "scientific" explanation than the apparatus by which an orchid is fertilised.

We can work back to the amoeba to find ourselves face to face with a scarcely organized mass of protoplasm. And then we find ourselves face to face with a problem which will, perhaps, forever remain insoluble scientifically. But as for that, so is the primeval material of which it (protoplasm) is composed. "Matter" itself is evaporating, for it is being resolved by physical research into something which is intangible.

We cannot form the slightest idea how protoplasm came into existence. It is impossible to regard it as a mere substance. It is a mechanism. Although the chemist may hope to make eventually all the substances which protoplasm fabricates, and will probably do so, he can only build them up by the most complicated processes. Protoplasm appears to be able to manufacture them straight off in a way of which the chemist cannot form the slightest conception. This is one aspect of the mystery of *life*. Herbert Spencer's definition tells one nothing.

Science can only explain nature as it reveals itself to the senses in terms of consciousness. The explanation may be all wrong in the eyes of omniscience. All one can say is that it is a practical working basis, and is good enough for mundane purposes. But if I am asked if I can solve the riddle of the Universe I can only answer, No. Brunetière then retorts that science is bankrupt. But this is equivocal. It only means that it cannot meet demands beyond its power to satisfy.

I entirely sympathise with anyone who seeks an answer from

some other non-scientific source. But I keep scientific explanations and spiritual craving wholly distinct.

The whole point of evolution, as formulated by Lyell and Darwin, is to explain phenomena by known causes. Now, directive power is not a known cause. Determination compels me to believe that every event is inevitable. If we admit a directive power, the order of nature becomes capricious and unintelligible. Excuse my saying all this. But that is the dilemma as it presents itself to *my* mind. If it does not trouble other people, I can only say, so much the better for them. Briefly, I am afraid I must say that it is ultra-scientific. I think that would have been pretty much Darwin's view.

I do not think that it is quite fair to say that biologists shirk the problem. In my opinion they are not called upon to face it. Bastian, I suppose, believed that he had bridged the gulf between lifeless and living matter. And here is a man, of whom I know nothing, who has apparently got the whole thing cut and dried.—Yours sincerely,

W. T. THISELTON-DYER.

TO PROF. POULTON

Old Orchard, Broadstone, Dorset. May 28, 1912.

My dear Poulton,—Thanks for your paper on Darwin and Bergson.¹ I have read nothing of Bergson's, and although he evidently has much in common with my own views, yet all vague ideas—like “an internal development force”—seem to me of no real value as an explanation of Nature.

I claim to have shown the necessity of an ever-present Mind as the primal cause both of all physical and biological evolution. This Mind works by and through the primal forces of nature—by means of Natural Selection in the world of Life; and I do not think I could read a book which rejects this method in favour of a vague “law of sympathy.” He might as well reject gravitation, electrical repulsion, etc., etc., as explaining the motions of cosmical bodies. . . .—Yours very truly,

ALFRED R. WALLACE.

¹ *Bedrock*, April, 1912, p. 48.

TO MR. BEN R. MILLER

Old Orchard, Broadstone, Dorset. January 18, 1913.

Dear Sir,—Thanks for your kind congratulations, and for the small pamphlet¹ you have sent me. I have read it with much interest, as the writer was evidently a man of thought and talent. The first lecture certainly gives an approach to Darwin's theory, perhaps nearer than any other, as he almost implies the "survival of the fittest" as the cause of progressive modification. But his language is imaginative and obscure. He uses "education" apparently in the sense of what we should term "effect of the environment."

The second lecture is even a more exact anticipation of the modern views as to microbes, including their transmission by flies and other insects and the probability that the blood of healthy persons contains a sufficiency of destroyers of the pathogenic germs—such as the white blood-corpuscles—to preserve us in health.

But he is so anti-clerical and anti-Biblical that it is no wonder he could not get a hearing in Boston in 1847.—Yours very truly,

ALFRED R. WALLACE.

TO PROF. POULTON

Old Orchard, Broadstone, Dorset. April 2, 1913.

My dear Poulton,—About two months ago an American . . . sent me the enclosed booklet,² which he had been told was very rare, and contained an anticipation of Darwinism.

This it certainly does, but the writer was highly imaginative, and, like all the other anticipators of Darwin, did not perceive the whole scope of his idea, being, as he himself says, not sufficiently acquainted with the facts of nature.

His anticipations, however, of diverging lines of descent from

¹ "Shall we have Common Sense? Some Recent Lectures." By George W. Sleeper. Boston, 1849.

² See footnote to preceding letter. The book formed the subject of Prof. Poulton's Presidential Addresses (May 24, 1913, and May 25, 1914) to the Linnean Society (*Proceedings*, 1912-13, p. 26, and 1913-14, p. 23). The above letter is in part quoted in the former address.

a common ancestor, and of the transmission of disease germs by means of insects, are perfectly clear and very striking.

As you yourself made known one of the anticipators of Darwin, whom he himself had overlooked, you are the right person to make this known in any way you think proper. As you have so recently been in America, you might perhaps ascertain from the librarian of the public library in Boston, or from some of your biological friends there, what is known of the writer and of his subsequent history.

If the house at Down is ever dedicated to Darwin's memory it would seem best to preserve this little book there; if not you can dispose of it as you think best.—Yours very truly,

ALFRED R. WALLACE.

P.S.—Two of my books have been translated into Japanese: will you ascertain whether the Bodleian would like to have them?

TO PROF. POULTON¹

Old Orchard, Broadstone, Dorset. June 3, 1913.

My dear Poulton,—I am very glad you have changed your view about the "Sleeper" lectures being a "fake." The writer was too earnest, and too clear a thinker, to descend to any such trick. And for what? "Agnostic" is not in Shakespeare, but it may well have been used by someone before Huxley. The parts of your Address of which you send me slips are excellent, and I am sure will be of great interest to your audience. I quite agree with your proposal that the "Lectures" shall be given to the Linnean Society.—Yours very truly,

ALFRED R. WALLACE.

TO MR. E. SMEDLEY

Old Orchard, Broadstone, Dorset. August 26, 1913.

Dear Mr. Smedley,—I am glad to see you looking so jolly. I return the photo to give to some other friend. Mr. Marchant,

¹ This letter relates to evidences, favourable to Sleeper, which had not at the time been critically examined, but broke down when carefully scrutinised. See Prof. Poulton's address to the Linnean Society, May 25, 1914 (*Proc.*, 1913-14, p. 23).

the lecturer you heard, is a great friend of mine, but is now less dogmatic. The Piltdown skull does not prove much, if anything!

The papers are wrong about me. I am not writing anything now; perhaps shall write no more. Too many letters and home business. Too much bothered with many slight ailments, which altogether keep me busy attending to them. I am like Job, who said "the grasshopper was a burthen" to him! I suppose its creaking song.—Yours very truly,

ALFRED R. WALLACE.

TO MR. W. J. FARMER

Old Orchard, Broadstone, Wimborne. 1913.

Dear Sir,— . . . I presume your question "Why?" as to the varying colour of individual hairs and feathers, and the regular varying of adjacent hairs, etc., to form the surface pattern, applies to the ultimate cause which enables those patterns to be hereditary, and, in the case of birds, to be reproduced after moulting yearly.

The purpose, or end they serve, I have, I think, sufficiently dealt with in my "Darwinism"; the method by which such useful tints and markings are produced, because useful, is, I think, clearly explained by the law of Natural Selection or Survival of the Fittest, acting through the universal facts of heredity and variation.

But the "why"—which goes further back, to the directing agency which not only brings each special cell of the highly complex structure of a feather into its exactly right position, but, further, carries pigments or produces surface striæ (in the case of the metallic or interference colours) also to their exactly right place, and nowhere else—is the mystery, which, if we knew, we should (as Tennyson said of the flower in the wall) "know what God and Man is."

The idea that "cells" are all conscious beings and go to their right places has been put forward by Butler in his wonderful book "Life and Habit," and now even Haeckel seems to adopt it. All theories of heredity, including Darwin's pangenesis, do not touch it, and it seems to me as fundamental as life and consciousness, and to be absolutely inconceivable by us till we know

what life is, what spirit is, and what matter is; and it is probable that we must develop in the spirit world some few thousand million years before we get to this knowledge—if then!

My book, "Man's Place in the Universe," shows, I think, indications of the vast importance of that Universe as the producer of Man which so many scientific men to-day try to belittle, because of what may be, in the infinite!—Yours very truly,

ALFRED R. WALLACE.

PART IV

Home Life

(By W. G. WALLACE and VIOLET WALLACE)

IN our father's youth and prime he was 6 ft. 1 in. in height, with square though not very broad shoulders. At the time to which our first clear recollections go back he had already acquired a slight stoop due to long hours spent at his desk, and this became more pronounced with advancing age; but he was always tall, spare and very active, and walked with a long easy swinging stride which he retained to the end of his life.

As a boy he does not appear to have been very athletic or muscularly strong, and his shortsightedness probably prevented him from taking part in many of the pastimes of his school-fellows. He was never a good swimmer, and he used to say that his long legs pulled him down. He was, however, always a good walker and, until quite late in life, capable of taking long country walks, of which he was very fond.

He was very quick and active in his movements at times, and even when 90 years of age would get up on a chair or sofa to reach a book from a high shelf, and move about his study with rapid strides to find some paper to which he wished to refer.

When out of doors he usually carried an umbrella, and in the garden a stick, upon which he leaned rather heavily in his later years. His hair became white rather early in life, but it remained thick and fine to the last, a fact which he attributed to always wearing soft hats. He had full beard and whiskers, which were also white. His eyes were blue and his complexion rather pale. He habitually wore spectacles, and to us he never looked quite natural without them. Towards the end of his life his eyes were subject to inflammation, and the glasses were blue. His hands, though large, were not clumsy, and were capable of very delicate manipulations, as is shown by his skill in handling

and preserving insects and bird-skins, and also in sketching, where delicacy of touch was essential. His handwriting is another example of this; it remained clear and even to the end, in spite of the fact that he wrote all his books, articles, and letters with his own hand until the last few years, when he occasionally had assistance with his correspondence; but his last two books, "Social Environment" and "The Revolt of Democracy," written when he was 90 years of age, were penned by himself, and the MSS. are perfectly legible and regular.

He was very domestic, and loved his home. His interest extended to the culinary art, and he was fond of telling us how certain things should be cooked. This became quite a joke among us. He was very independent, and it never seemed to occur to him to ask to have anything done for him if he could do it himself—and he could do many things, such as sewing on buttons and tapes and packing up parcels, with great neatness. When unpacking parcels he never cut the string if it could be untied, and he would fold it up before removing the paper, which in its turn was also neatly folded.

His clothes were always loose and easy-fitting, and generally of some quiet-coloured cloth or tweed. Out of doors he wore a soft black felt hat rather taller than the clerical pattern, and a black overcoat unless the weather was very warm. He wore no ornaments of any kind, and even the silver watch-chain was worn so as to be invisible. He wore low collars with turned-down points and a narrow black tie, which was, however, concealed by his beard. He was not very particular about his personal appearance, except that he always kept his hair and beard well brushed and trimmed.

In our early days at Grays we children were allowed to run in and out of his study; but if he was busy writing at the moment we would look at a book until he could give us his attention. His brother in California sent him a live specimen of the lizard called the "horned toad," and this creature was kept in the study, where it was allowed to roam about, its favourite place being on the hearth.

About this time he read "Alice through the Looking-glass," which pleased him greatly; he was never tired of quoting from it and using some of Lewis Carroll's quaint words till it became one of our classics.

Some of our earliest recollections are of the long and interesting walks we took with our father and mother. He never failed to point out anything of interest and tell us what he knew about it, and would answer our numerous questions if possible, or put us off with some joking reference to Boojums or Jabberwocks. We looked upon him as an infallible source of information, not only in our childhood, but to a large extent all his life. When exploring the country he scorned "trespass boards." He read them "Trespassers will be persecuted," and then ignored them, much to our childish trepidation. If he was met by indignant gamekeepers or owners, they were often too much awed by his dignified and commanding appearance to offer any objection to his going where he wished. He was fond of calling our attention to insects and to other objects of natural history, and giving us interesting lessons about them. He delighted in natural scenery, especially distant views, and our walks and excursions were generally taken with some object, such as finding a bee-orchis or a rare plant, or exploring a new part of the country, or finding a waterfall.

In 1876 we went to live at Dorking, but stayed there only a year or two. An instance of his love of mystifying us children may be given. It must have been shortly after our arrival at Dorking that one day, having been out to explore the neighbourhood, he returned about tea-time and said, "Where do you think I have been? To Glory!" Of course we were very properly excited, and plied him with questions, but we got nothing more out of him then. Later on we were taken to see the wonderful place called "Glory Wood"; and it had surely gained in glory by such preparation.

Sometimes it would happen that a scene or object would recall an incident in his tropical wanderings and he would tell us of the sights he had seen. At the time he was greatly interested in botany, in which he was encouraged by our mother, who was an ardent lover of flowers; and to the end of his life he exhibited almost boyish delight when he discovered a rare plant. Many walks and excursions were taken for the purpose of seeing some uncommon plant growing in its natural habitat. When he had found the object of his search we were all called to see it. During his walks and holidays he made constant use of the one-inch Ordnance Maps, which he obtained for each dis-

trict he visited, planning out our excursions on the map before starting. He had a gift for finding the most beautiful walks by means of it.

In 1878 we moved to Croydon, where we lived about four years. It was at this time that he hoped to get the post of Superintendent of Epping Forest. We still remember all the delights we children were promised if we went to live there. We had a day's excursion to see the Forest, he with his map finding out the roads and stopping every now and then to admire a fresh view or to explain what he would do if the opportunity were given him. It was a very hot day, and we became so thirsty that when we reached a stream, to our great joy and delight he took out of his pocket, not the old leather drinking-cup he usually carried, but a long piece of black india-rubber tubing. We can see him now, quite as pleased as we were with this brilliant idea, letting it down into the stream and then offering us a drink! No water ever tasted so nice! Our mother used to be a little anxious as to the quality of the water, but he always put aside such objections by saying *running* water was quite safe, and somehow we never came to any harm through it. The same happy luck attended our cuts and scratches; he always put "stamp-paper" on them, calling it plaster, and we knew of no other till years later. He used the same thing for his own cuts, etc., to the end of his life, with no ill effects.

In 1881 we moved again, this time to Godalming, where he had built a small house which he called "Nutwood Cottage." After Croydon this was a very welcome change and we all enjoyed the lovely country round. The garden as usual was the chief hobby, and Mr. J. W. Sharpe, our old friend and neighbour in those days, has written his reminiscences of this time which give a very good picture of our father. They are as follows:

"About thirty-five years ago Dr. Wallace built a house upon a plot of ground adjoining that upon which our house stood. I was at that time an assistant master at Charterhouse School; and Dr. Wallace became acquainted with a few of the masters besides myself. With two or three of them he had regular weekly games of chess; for he was then and for long afterwards very fond of that game; and, I understand, possessed considerable skill at it. A considerable portion of his spare time was spent

in his garden, in the management of which Mrs. Wallace, who had much knowledge and experience of gardening, very cordially assisted him. Here his characteristic energy and restlessness were conspicuously displayed. He was always designing some new feature, some alteration in a flower-bed, some special environment for a new plant; and always he was confident that the new schemes would be found to have all the perfections which the old ones lacked. From all parts of the world botanists and collectors sent him, from time to time, rare or newly discovered plants, bulbs, roots or seeds, which he, with the help of Mrs. Wallace's practical skill, would try to acclimatise, and to persuade to grow somewhere or other in his garden or conservatory. Nothing disturbed his cheerful confidence in the future, and nothing made him happier than some plan for reforming the house, the garden, the kitchen boiler, or the universe. And, truth to say, he displayed great ingenuity in all these enterprises of reformation. Although they were never in effect what they were expected to be by their ingenious author, they were often sufficiently successful; but, successful or not, he was always confident that the next would turn out to be all that he expected of it. With the same confidence he made up his mind upon many a disputable subject; but, be it said, never without a laborious examination of the necessary data, and the acquisition of much knowledge. In argument, of which intellectual exercise he was very fond, he was a formidable antagonist. His power of handling masses of details and facts, of showing their inner meaning and the principles underlying them, and of making them intelligible, was very great; and very few men of his time had it in equal measure.

"But the most striking feature in his conversation was his masterly application of general principles: these he handled with extraordinary skill. In any subject with which he was familiar, he would solve, or suggest a plausible solution of, difficulty after difficulty by immediate reference to fundamental principles. This would give to his conclusions an appearance of inevitableness which usually overbore his adversary, and, even if it did not convince him, left him without any effective reply. This, too, had a good deal to do, I am disposed to conjecture, with another very noticeable characteristic of his which often came out in conversation, and that was his apparently

unfailing confidence in the goodness of human nature. No man nor woman but he took to be in the main honest and truthful, and no amount of disappointment—not even losses of money and property incurred through this faith in others' virtues—had the effect of altering this mental habit of his.

"His intellectual interests were very widely extended, and he once confessed to me that they were agreeably stimulated by novelty and opposition. An uphill fight in an unpopular cause, for preference a thoroughly unpopular one, or any argument in favour of a generally despised thesis, had charms for him that he could not resist. In his later years, especially, the prospect of writing a new book, great or small, upon any one of his favourite subjects always acted upon him like a tonic, as much so as did the project of building a new house and laying out a new garden. And in all this his sunny optimism and his unfailing confidence in his own powers went far towards securing him success."—J. W. S.

"Land Nationalisation" (1882), "Bad Times" (1885), and "Darwinism" (1889) were written at Godalming, also the series of lectures which he gave in America in 1886–7 and at various towns in the British Isles. He also continued to have examination papers¹ to correct each year—and a very strenuous time that was. Our mother used to assist him in this work, and also with the indexes of his books.

We now began to make nature collections, in which he took the keenest interest, many holidays and excursions being arranged to further these engrossing pursuits. One or two incidents occurred at "Nutwood" which have left clear impressions upon our minds. One day one of us brought home a beetle, to the great horror of the servant. Passing at the moment, he picked it up, saying, "Why, it is quite a harmless little creature!" and to demonstrate its inoffensiveness he placed it on the tip of his nose, whereupon it immediately bit him and even drew blood, much to our amusement and his own astonishment. On another occasion he was sitting with a book on the lawn under the oak tree when suddenly a large creature alighted upon his shoulder. Looking round, he saw a fine specimen of the ring-

¹ For many years he was Examiner in Physiography at South Kensington.

tailed lemur, of whose existence in the neighbourhood he had no knowledge, though it belonged to some neighbours about a quarter of a mile away. It seemed appropriate that the animal should have selected for its attentions the one person in the district who would not be alarmed at the sudden appearance of a strange animal upon his shoulder. Needless to say, it was quite friendly.

A year or so before we left Godalming he enlarged the house and altered the garden. But his health not having been very good, causing him a good deal of trouble with his eyes, and having more or less exhausted the possibilities of the garden, he decided to leave Godalming and find a new house in a milder climate. So in 1889 he finally fixed upon a small house at Parkstone in Dorset.

Planning and constructing houses, gardens, walls, paths, rockeries, etc., were great hobbies of his, and he often spent hours making scale drawings of some new house or of alterations to an existing one, and scheming out the details of construction. At other times he would devise schemes for new rockeries or waterworks, and he would always talk them over with us and tell us of some splendid new idea he had hit upon. As Mr. Sharpe has noted, he was always very optimistic, and if a scheme did not come up to his expectations he was not discouraged, but always declared he could do it much better next time and overcome the defects. He was generally in better health and happier when some constructional work was in hand. He built three houses, "The Dell" at Grays, "Nutwood Cottage" at Godalming, and the "Old Orchard" at Broadstone. The last he actually built himself, employing the men and buying all the materials, with the assistance of a young clerk of works; but though the enterprise was a source of great pleasure, it was a constant worry. He also designed and built a concrete garden wall, with which he was very pleased, though it cost considerably more than he anticipated. He had not been at Parkstone long before he set about the planning of "alterations" with his usual enthusiasm. We were both away from home at this time, and consequently had many letters from him, of which one is given as a specimen. His various interests are nearly always referred to in these letters, and in not a few of them his high spirits show themselves in bursts of exuberance which were very characteristic whenever a new scheme was afoot.

The springs of eternal youth were forever bubbling up afresh, so that to us he never grew old. One of us remembers how, when he must have been about 80, someone said, "What a wonderful old man your father is!" This was quite a shock, for to us he was not old. The letter referred to above is the following:

TO MR. W. G. WALLACE

Parkstone, Dorset. February 1, 1891.

My dear Will,—Another week has passed away into eternity, another month has opened its eyes on the world, and still the illustrious Charles [bricklayer] potters about, still the carpenter plies the creaking saw and the stunning hammer, still the plumber plumbs and the bellhanger rattles, still the cisterns overflow and the unfinished drains send forth odorous fumes, still the rains descend and all around the house is a muddle of muck and mire, and still there is so much to do that we look forward to some far distant futurity, when all that we are now suffering will be over, and we may look back upon it as upon some strange yet not altogether uninteresting nightmare!

Briefly to report progress. The new pipe-man has finished the bathroom and nearly done the bells, and we have had gas alight the last three days. The balcony is finished, the bath and laboratory are closed up and waiting for the varnishers. Charles has finished the roof, and the scaffolding is removed. But though two plumbers have tried all their skill, the ball-cock in the cistern won't work, and when the water has been turned on an hour it overflows. The gutters and pipes to roof are not up, and the night before last a heavy flood of rain washed a quantity of muddy water into the back entrance, which flowed right across the kitchen into the back passage and larder, leaving a deposit of alluvial mud that would have charmed a geologist. However, we have stopped that for the future by a drain under the doorstep. The new breakfast-room is being papered and will look tidy soon. A man has been to measure for the stairs. The front porch door is promised for to-morrow, and the stairs, I suppose, in another week. A lot of fresh pointing is to be done, and all the rain-water pipes and the rain-water cistern with its overflow pipes, and then the greenhouse, and then all the outside painting—after which we shall rest for a month and then do the

inside papering; but whether that can be done before Easter seems very doubtful. . . .

Our alterations still go on. The stairs just up—Friday night we had to go outside to get to bed, and Saturday and Sunday we *could* get up, but over a chasm, and with alarming creaks. Now it is all firm, but no handrail yet. Painters still at work, and whitewashers. Porch door up, with two birds in stained glass—looks fine—proposed new name, “Dicky-bird Lodge.” Bath fixed, but waiting to be varnished—luxurious! . . .

Dr. Wallace had already received four medals from various scientific societies, and at our suggestion he had a case made to hold them all, which is referred to in the following letter. The two new medals mentioned were those of the Royal Geographical and Linnean Societies. He attached very little importance to honours conferred upon himself, except in so far as they showed acceptance of “the truth” as he called it.

TO MISS VIOLET WALLACE

Parkstone, Dorset. April 3, 1892.

My dear Violet,— . . . I have got J. G. Wood’s book on the horse. It is very good; I think the best book he has written, as his heart was evidently in it. . . .

A dreadful thing has happened! Just as I have had my medal-case made, “regardless of expense,” they are going to give me another medal! Hadn’t I better decline it, with thanks? “No room for more medals”!!—Your affectionate papa,

ALFRED R. WALLACE.

P.S.—A poor man came here last night (Saturday) with a basket of primrose roots—had carried them eight miles, couldn’t sell one in Poole or Parkstone—was 64 years old—couldn’t get any work to do—had no home, etc. So, though I do not approve of digging up primrose roots as a trade, I gave him 1s. 6d. for them, pitying him as one of the countless victims of Landlordism.—A. R. W.

A poor man was sentenced to fourteen days’ hard labour last week for picking snowdrops in Charborough Park. Shame!—A. R. W., Pres. L. N. Society.

TO MISS VIOLET WALLACE

Parkstone, Dorset. May 5, 1892.

My dear Violet,—I have finished reading “Freeland.” It is very good—as good a story as “Looking Backward,” but not quite so pleasantly written—rather heavy and Germanic in places. The results are much the same as in “Looking Backward” but brought about in a different and very ingenious manner. It may be called “Individualistic Socialism.” I shall be up in London soon, I expect, to the first Meetings of the Examiners in the great science of “omnium gatherum.”¹—Your affec. papa,

ALFRED R. WALLACE.

While he lived at Parkstone our father built a small orchid house in which he cultivated a number of orchids for a few years, but the constant attention which they demanded, together with the heated atmosphere, were too much for him, and he was obliged to give them up. He was never tired of admiring their varied forms and colours, or explaining to friends the wonderful apparatus by which many of them were fertilised. The following letter shows his enthusiasm for orchids:

TO MISS VIOLET WALLACE

Parkstone, Dorset. November 25, 1894.

My dear Violet,— . . . I have found a doctor at Poole (Mr. Turner) who has two nice orchid houses which he attends to entirely himself, and as I can thus get advice and sympathy from a fellow maniac (though he *is* a public vaccinator!) my love of orchids is again aroused to fever-heat, and I have made some alterations in the greenhouse which will better adapt it for orchid growing, and have bought a few handsome kinds very cheap, and these give me a lot of extra work and amusement. . . .

TO HIS WIFE

Hôtel du Glacier du Rhône. Wednesday evening, [July, 1895].

My dear Annie,—I send you now a box of plants I got on both sides of the Furka Pass yesterday, and about here to-day.

¹ See footnote on p. 354.

The Furka Pass on both sides is a perfect flower-garden, and the two sides have mostly different species. The violets and anemones were lovely, and I have got two species of glorious gentians. . . . All the flowers in the box are very choice species, and have been carefully dug up, and having seen how they grow, I have been thinking of a plan of making a little bed for them on the top of the new rockery where there is now nothing particular. Will you please plant them out carefully in the zinc tray of peat and sphagnum that stands outside near the little greenhouse door? Just lift up the sphagnum and see if the earth beneath is moist, if not give it a soaking. Then put them all in, the short-rooted ones in the sphagnum only, the other through into the peat. Then give them a good syringing and put the tray under the shelf outside the greenhouse, and cover with newspaper for a day or two. After that I think they will do, keeping them moist if the weather is dry. I am getting hosts of curiosities. To-day we found four or five species of willows from $\frac{1}{4}$ in. to 2 in. high, and other rarities. . . . In haste for post and dinner.—Your ever affectionate,

ALFRED R. WALLACE.

TO MISS VIOLET WALLACE

Parkstone, Dorset. October 22, 1897.

My dear Violet,—In your previous letter you asked me the conundrum, Why does a wagtail wag his tail? That's quite easy, on Darwinian principles. Many birds wag their tails. Some Eastern flycatchers—also black and white—wag their long tails up and down when they alight on the ground or on a branch. Other birds with long tails jerk them up in the air when they alight on a branch. Now these varied motions, like the motions of many butterflies, caterpillars, and many other animals, must have a use to the animal, and the most common, or rather the most probable, use is, either to frighten or to distract an enemy. If a hawk was very hungry and darted down on a wagtail from up in the air, the wagging tail would be seen most distinctly and be aimed at, and thus the bird would be missed or at most a feather torn out of the tail. The bird hunts for food in the open, on the edges of ponds and streams, and would be especially

easy to capture, hence the wagging tail has been developed to baffle the enemy. . . .

TO MISS VIOLET WALLACE

Parkstone, Dorset. March 8, 1899.

My dear Violet,— . . . I have now finished reading the "Maha Bharata," which is on the whole very fine—finer, I think, than the "Iliad." I have read a good deal of it twice, and it will bear reading many times. It corresponds pretty nearly in date with the "Iliad," the scenes it describes being supposed to be about B.C. 1500. Many of the ideas and moral teachings are beautiful; equal to the best teaching and superior to the general practice of to-day. I have made a lot of emendations and suggestions, which I am going to send to the translator, as the proofs have evidently not been carefully read by any English literary man.

About the year 1899 Dr. Wallace began to think of leaving Parkstone, partly for reasons of health and partly to get a larger garden, if possible. He spent three years in looking for a suitable spot in many of the southern counties, and we were all pressed to join in the search. Finally he found just the spot he wanted at Broadstone, only three miles away.

The following letters describe his final success—all written with his usual optimism and high spirits:

TO MR. W. G. WALLACE

Parkstone, Dorset. October 26, 1901.

My dear Will,—At length the long quest has come to an end, and I have agreed to buy three acres of land at Broadstone. Ma and I have just been over again this morning to consider its capabilities, and the exact boundaries that will be the most advantageous, as I have here the great advantage of choosing exactly what I will have. I only wish I could afford five acres instead of three, or even ten; but the three will contain the very eye of the whole. I enclose you a bit of the 6-inch ordnance on which I have marked the piece I have finally fixed upon in red chalk. The attractive bit is the small enclosure of one acre,

left rather paler, which is an old orchard in a little valley sloping downward to the S.S.E. There are, perhaps, a score of trees in it—apples, pears, plums and cherries, I believe, and under them a beautiful green short turf like a lawn—kept so, I believe, by rabbits. From the top of this orchard is a fine view over moor and heather, then over the great northern bay of Poole Harbour, and beyond to the Purbeck Hills and out to the sea and the Old Harry headland. It is not very high—about 140 feet, I think, but being on the edge of one of the plateaus the view is very effective. On the top to the left of the road track is a slightly undulating grass field, of which I have a little less than an acre. To the right of the fence, and coming down to the wood, is very rough ground densely covered with heather and dwarf gorse, a great contrast to the field. The wood on the right is mixed but chiefly oak, I think, with some large firs, one quite grand; while the wood on the left is quite different, having some very tall Spanish chestnuts loaded with fruit, some beeches, some firs—but I have not had time yet to investigate thoroughly. Thus this little bit of three acres has five subdivisions, each with a quite distinct character of its own, and I never remember seeing such variety in such a small area. The red wavy line is about where I shall have to make my road, for the place has now no road, and I think I am very lucky in discovering it and in getting it. Another advantage is in the land, which is varied to suit all crops. I fancy . . . I shall find places to grow most of my choice shrubs, etc., better than here. I expect bulbs of all kinds will grow well, and I mean to plant a thousand or so of snowdrops, crocuses, squills, daffodils, etc., in the orchard, where they will look lovely.

TO MR. W. G. WALLACE

Parkstone, Dorset. November 6, 1901.

My dear Will,— . . . I have taken advantage of a foggy cold day to trace you a copy of the ground plan of the proposed house. . . . Of course the house will be much larger than we want, but I look to future value, and rather than build it smaller, to be enlarged afterwards, I would prefer to leave the drawing-room and bedroom adjoining with bare walls inside till they can be properly finished. The housekeeper's room would be a nice

dining-room, and the hall a parlour and drawing-room combined. But the outside must be finished, on account of the garden, creepers, etc. The S.E. side (really about S.S.E.) has the fine views. If you can arrange to come at Christmas we will have a picnic on the ground the first sunny day. I was all last week surveying—a very difficult job, to mark out exactly three acres so as to take in exactly as much of each kind of ground as I wanted, and with no uninterrupted view over any one of the boundary lines! I found the sextant, and it was very useful setting out the two right angles of the northern boundary. I have not got possession yet, but hope to do so by next week. The house, we reckon, can be built for £1,000 at the outside. . . .

TO MRS. FISHER

Parkstone, Dorset. February 4, 1902.

Dear Mrs. Fisher,— . . . You will be surprised to hear that I have been so rash as to buy land and to (propose to) build a house! Every other effort to get a pleasant country cottage with a little land having failed, we discovered, accidentally, a charming spot only four miles from this house and half a mile from Broadstone Station, and have succeeded in buying three acres, *chosen by myself*, from Lord Wimborne at what is really a reasonable price. In its contour, views, wood, and general aspect of wild nature it is almost perfection; and Annie, Violet, and Will are all pleased and satisfied with it. It is on the slope of the Broadstone middle plateau, looking south over Poole Harbour with the Purbeck Hills beyond, and a little eastward out to the sea. . . . The ground is good loam in the orchard, with some sand and clay in the field, but this is so open to the sun and air that we are not afraid of it, as the *house-site* will be entirely concreted over, and I have arranged for a heating stove in a cellar, which will warm and dry the whole basement. In a week or two we hope to begin building, so you may fancy how busy I am, especially as we are building it without a contractor, with the help of a friend. . . . I go over two or three times a week, as I have two gardeners at work. In the summer (should I be still in the land of the living) I hope you will be able to come and see our little estate, which is to be called by the descriptive name of "Old Orchard." I have got a good architect to make the work-

ing drawings and he has designed a very picturesque yet unpretentious house.—Yours very truly,

ALFRED R. WALLACE.

TO MR. W. G. WALLACE

Parkstone, Dorset. March 2, 1902.

My dear Will,—This week's progress has been fairly good although the wet after the frost has caused two falls in the cellar excavations, and we have had to put drain pipes to carry water out, though not much accumulated. . . . During the week some horses in the field have not only eaten off the tops of the privet hedge, but have torn up some dozens of the plants by the roots, by putting their heads over the 4-foot wire fence. I am therefore obliged in self-defence to raise the post a foot higher and put barbed wire along the top of it. Some cows also got in our ground one day and ate off the tops of the newly planted laurels, which I am told they are very fond of, so I have got a chain and padlock for our gate. . . .

We moved into the new house at Broadstone at the end of November, 1902, before it was quite finished, and here Dr. Wallace lived till the end of his life. The garden was an endless source of interest and occupation, being much larger than any he had had since leaving Grays.

When writing he was not easily disturbed and never showed any impatience or annoyance at any interruption. If interrupted by a question he would pause, pen in hand, and reply or discuss the matter and then resume his unfinished sentence.

He seemed to have the substance of his writing in his mind before he commenced, and did not often refer to books or to notes, though he usually had one or two books or papers on the table at hand, and sometimes he would jump up to get a book from the shelves to verify some fact or figure. When preparing for a new book or article he read a great many works and papers bearing on the subject. These were marked with notes and references on the flyleaves, and often by pencil marks to indicate important passages, but he did not often make separate notes. He had a wonderful memory, and stored in his mind the facts and arguments he wished to use, or the places where they were

to be found. He borrowed many books from libraries, and from these he sometimes made a few notes. He was not a sound sleeper, and frequently lay awake during the night, and then it was that he thought out and planned his work. He often told us with keen delight of some new idea or fresh argument which had occurred to him during these waking hours.

After spending months, or sometimes years, in reading and digesting all the literary matter he could obtain on a subject, and forming a plan for the treatment of it, he would commence writing, and keep on steadily for five or six hours a day if his health permitted. He also wrote to people all over the world to obtain the latest facts bearing on the subject.

In 1903 he began writing "Man's Place in the Universe."

TO MR. W. G. WALLACE

Old Orchard. July 8, 1903.

My dear Will,—I have just finished going over your notes and corrections of the last four chapters. I can't think how I was so stupid to make the mistake in figures which you corrected. In almost all cases I have made some modification in accordance with your suggestions, and the book will be much improved thereby. I have put in a new paragraph about the stars in other parts than the Milky Way and Solar Cluster, but there is really nothing known about them. I have also cut out the first reference to Jupiter altogether. Of course a great deal is speculative, but any reply to it is equally speculative. The question is, which speculation is most in accordance with the known facts, and not with prepossessions only?

Considering that the book has all been read up and written in less than three months, it cannot be expected to be as complete and careful as if three years had been expended on it, but then it is fresher perhaps. The bit about the pure air came to me while writing, and I let myself go. Why should I not try and do a little good and make people think a little on such matters, when I have the chance of perhaps more readers than all my other books?

As to my making too much of Man, of course that is the whole subject of the book! And I look at it differently from you, because I know *facts* about him you neither know nor believe *yet*.

If you are once convinced of the facts and teachings of Spiritualism, you will think more as I do.

The following letter refers to his little book on Mars.

TO MR. W. G. WALLACE

Broadstone, Wimborne. September 26, 1907.

My dear Will,— . . . After elaborate revision and correction I have sent my MS. of the little "Mars" book to Macmillans yesterday. . . . Will you read the whole proofs carefully, in the character of the "intelligent reader"? Your fresh eye will detect little slips, bad logic, too positive statements, etc., which I may have overlooked. It will only be about 100 or 150 pages large type—and I want it to be really good, and free from blunders that any fool can see. . . .

For some years now he had suffered from repeated attacks of asthma and bronchitis. He had tried the usual remedies for these complaints without any good results, and, though still able to write, had then no thought of beginning any large work; in fact, he considered he had but a few more years to live. When Mr. Bruce-Joy came to see him in order to model the portrait medallion, he mentioned in the course of conversation that he had tried the Salisbury treatment with wonderful results. Our father was at first incredulous, but decided to try it in a modified form. He gave up all starchy foods and ate beef only, cooked in a special manner to render it more digestible. He found such relief from this change of diet that from this time onwards he followed a very strict daily routine, which he continued to the end of his life with slight variations.

He made himself a cup of tea on a gas stove in his bedroom at 6 a.m. (the exact quantity of tea and water having been measured the previous evening), and boiled it in a small double saucepan for a definite time by the watch. He always said this cup of tea tasted better than at any other time of the day. He then returned to bed and slept till 8 a.m. During his last two or three years he suffered from rheumatism in his shoulder and it took him a long time to dress, and he called in the aid of his gardener in the last year, who acted as his valet. While dress-

ing he prepared a cup of cocoa on the gas stove, which he carried into the study (next door) at 9 a.m. This was all he had for breakfast, and he took it while reading the paper or his letters.

Dinner at one o'clock was taken with his family, and he usually related any interesting or striking news he had read in the paper, or in his correspondence, and commented upon it, or perhaps he would tell us of some new flower in the garden.

He drank hot water with a little Canary sack and a dash of soda-water, to which he added a spoonful of plum jam. He was very fond of sweet things, such as puddings, but he had to partake sparingly of them, and it was a great temptation when some dish of which he was particularly fond was placed upon the table.

After dinner he usually took a nap in the study before resuming work or going into the garden.

Tea was at four o'clock, and consisted only of a cup of tea, which he made himself in the study, unless there were visitors whom he wished to see, when he would sometimes take it into the drawing-room and make it there.

After tea he again wrote, or took a turn in the garden if the weather and season permitted. Latterly he spent a good part of the afternoon and evening reading and dozing on the sofa, and only worked at short intervals when he felt equal to it.

Supper, at seven, was a repetition of dinner, and he took it with us in the dining-room. After supper he generally read a novel before the fire except in the very hottest weather, and he frequently dozed on and off till he retired at eleven. He made himself a cup of cocoa while preparing for bed, and drank it just before lying down.

For the last year or two it was a constant difficulty with him to secure enough nourishment without aggravating his ailments by indigestion. During this time he suffered continuous discomfort, though he seldom gave utterance to complaint or allowed it to affect the uniform equability of his temper.

In 1903 his daughter came to live with her parents, who generously allowed her to take three or four children as pupils. At first we feared they might bother our father, but he really enjoyed seeing them about and talking to them. He was always interested in any new child, and if for a short time none were

forthcoming, always lamented the fact. At dinner the children would ask him all sorts of questions, very amusing ones sometimes. They were also intensely interested in what he ate, and watched with speechless wonder when they saw him eating orange, banana, and sugar with his meat.

One of these early pupils, Reginald B. Rathbone, has sent reminiscences which are so characteristic that we give them as they stand:

"I have stayed at Dr. Wallace's house on three occasions; the first two were when I was only about eight or nine years old, and my recollections of him at that time are therefore necessarily somewhat dim. Certain things, however, have stuck in my memory. I went there quite prepared to see a very venerable and imposing-looking old gentleman, and filled in advance with much awe and respect for him. As regards his personal appearance I was by no means disappointed, as his tall, slightly-stooping figure, long white hair and beard, and his spectacles fulfilled my highest expectations. I remember being struck with the kindly look of his eyes, and indeed they did not belie his nature, for he always treated me with great kindness, patience and indulgence, which is somewhat remarkable considering my age, and how exasperating I must have been sometimes. I soon began to regard him as a never-failing fount of wisdom, and as one who could answer any question one liked to put to him. Of this latter fact I was not slow to take advantage. I plied him with every kind of question my imaginative young brain could conceive, usually beginning with 'why.'

"He nearly always gave me an answer, and what is more, a satisfactory one, and well within the scope of my limited understanding. These definite, satisfactory answers of his used to afford me great pleasure, it being quite a new experience for me to have all my questions answered for me in this way. These answers, as I have said, were nearly always forthcoming, though indeed, on one or two occasions, in answer to an especially ridiculous query of mine he would answer, 'That is a very foolish question, Reggie.' But this was very rare.

"I remember taking a great interest in what Dr. Wallace ate. He had a hearty appetite, and was no believer in vegetarianism, for at lunch his diet consisted chiefly of cold beef,

liberally seasoned with various sauces and relishes, also vinegar. I used to gaze at these bottles with great admiration. Whenever there were peas he used to take large quantities of sugar with them. This greatly aroused my curiosity, and I questioned him about it. 'Why,' said he, 'peas themselves contain sugar; it is, therefore, much more sensible to take sugar with them than salt.' And he recounted an anecdote of how an eminent personage he had once dined with had been waited on with great respect and attention by all present, but salt was offered to him with the peas. 'If you want to make me quite happy,' said the great man, 'you will give me some sugar with my peas.' His favourite drink, I remember, was Canary sack.

"He had a strongly humorous side, and always enjoyed a good laugh. As an instance of this, I will recount the following incident: When I had returned home after my first visit to 'The Old Orchard,' my sister, three years older than myself, and I had a heated argument on the subject of the number of stomachs in a cow. I insisted it was three, she, on the other hand, held that it was seven. After a long and fierce dispute, I exclaimed: 'Well, let us write to Dr. Wallace, and he will settle it for us and tell us the real number.' This we did, the brazen audacity of the proceeding not striking us at the time. By return of post we received a letter which, alas! I have unfortunately not preserved, but the substance of which I well remember. 'Dear Irene and Reggie,' it ran, 'Your dispute as to the number of stomachs which a cow possesses can be settled and rectified by a simple mathematical process usually called substraction, thus:

'Irene's Cow.....	7 stomachs
Reggie's Cow.....	3 stomachs

The Farmer's Cow.....4 stomachs.'

"Dr. Wallace then went on to explain the names and uses of the four stomachs.

"Two instances of his fun come to my mind as I write. 'Why,' I asked, 'do you sometimes take off your spectacles to read the paper?' 'Because I can see better without 'em,' he said. 'Then why,' I asked again, 'do you ever wear them?' 'Because I can see better with 'em,' was the reply. The other instance relates to chloroform. He was describing the agonies suffered by those

who had to undergo amputation before the discovery of anæsthetics, whereas nowadays, he said, 'you are put under chloroform, then wake up and find your arm cut off, having felt nothing. Or you wake up and find your leg cut off. Or you wake up and find your head cut off!' He then laughed heartily at his own joke.

"These are just a few miscellaneous reminiscences, many of them no doubt trivial, but they may perhaps be not entirely devoid of interest, when it is remembered that they are the impressions and recollections of one who was then a boy of eight years old."—R. B. R.

The year 1908 was very auspicious to Dr. Wallace. To begin with, it was the fiftieth anniversary of the reading of the Darwin and Wallace joint papers on the Origin of Species before the Linnean Society, an event which was commemorated in the way described elsewhere.

In the autumn, and just as he was beginning to recover from a spell of bad health, he was invited to give a lecture at the Royal Institution, the prospect of which seemed to have upon him a most stimulating effect; he at once began to think about a suitable subject.

Following closely on this came the news that the Order of Merit was to be conferred upon him. His letters to his son give the details of this eventful period:¹

TO MR. W. G. WALLACE

Old Orchard, Broadstone, Wimborne. October 28, 1908.

My dear Will,— . . . I have a rather surprising bit of news for you. When I was almost at my worst, feeling very bad, I had a letter inviting me to give an evening lecture at the Royal Institution, for their Jubilee of the "Origin of Species"! Of course I decided at once to decline as impossible, etc., having nothing new to say, etc. But a few hours afterwards an idea suddenly came to me for a very fine lecture, if I can work it out as I hope—and the more I thought over it the better it seemed. So, two days back, I wrote to Sir W. Crookes—the Honorary Secretary,

¹ For letters from Wallace describing Col. Legge's visit with the Order, see pp. 370 and 448.

who had written to me—accepting provisionally! . . . Here is another “crowning honour”—the most unexpected of all! . . .

TO MR. W. G. WALLACE

Old Orchard, Broadstone, Wimborne. December 2, 1908.

My dear Will,— . . . This morning the Copley Medals came, gold and silver, smaller than any of the others, but very beautifully designed; the face has the Royal Society's arms, with Copley's name, and “Dignissimo,” and my name below. The reverse is the Royal Arms. By the same post came a letter from the Lord Chancellor's Office informing me, to my great relief, that the King had been graciously pleased to dispense with my personal attendance at the investiture of the Order of Merit. . . .

TO MR. W. G. WALLACE

Old Orchard, Broadstone, Wimborne. December 17, 1908.

My dear Will,—The ceremony is over, very comfortably. I am duly “invested,” and have got two engrossed documents, both signed by the King, one appointing me a member of the “Order of Merit” with all sorts of official and legal phrases, the other a dispensation from being personally “invested” by the King—as Col. Legge explained, to safeguard me as having a right to the Order in case anybody says I was not “invested.” . . . Colonel Legge was a very pleasant, jolly kind of man, and he told us he was in attendance on the German Emperor when he was staying near Christchurch last summer, and went for many drives with the Emperor only, all about the country. Col. Legge got here at 2.40, and had to leave at 3.20 (at station), so we got a carriage from Wimborne to meet the train and take him back, and Ma gave him some tea, and he said he had got a nice little place at Stoke Poges but with no view like ours, and he showed me how to wear the Order and was very pleasant: and we were all pleased. . . .

The next letter refers to the discovery of a rare moth and some beetles in the root of an orchid. It was certainly a strange yet pleasant coincidence that these creatures should find themselves in Dr. Wallace's greenhouse, where alone they would be noticed and appreciated as something uncommon.

TO MR. W. G. WALLACE

Old Orchard, Broadstone, Wimborne. February 23, 1909.

My dear Will,— . . . In my last letter I did not say anything about my morning at the Nat. Hist. Museum. . . . What I enjoyed most was seeing some splendid New Guinea butterflies which Mr. Rothschild¹ and his curator, Mr. Jordan, brought up from Tring on purpose to show me. I could hardly have imagined anything so splendid as some of these. I also saw some of the new paradise birds in the British Museum. But Mr. Rothschild says they have five times as many at Tring, and much finer specimens, and he invited me to spend a week-end at Tring and see the Museum. So I may go, perhaps—in the summer. But I have a curious thing to tell you about insect collecting at “Old Orchard.” About five months back I was examining one of the clumps of an orchid in the glass case—which had been sent me from Buenos Ayres by Mr. John Hall—when three pretty little beetles dropped out of it, on the edge of the tank, and I only managed to catch two of them. They were pretty little Longicornes, about an inch long, but very slender and graceful, though only of a yellowish-brown colour. I sent them up to the British Museum asking the name, and telling them they could keep them if of any use. They told me they were a species of the large South American genus *Ibidion*, but they had not got it in the collection!

On the Sunday before Christmas Day I was taking my evening inspection of the orchids, etc., in the glass case when a largish insect flew by my face, and when it settled it looked like a handsome moth or butterfly. It was brilliant orange on the lower wings, the upper being shaded orange brown, very moth-like, but the antennæ were clubbed like a butterfly's. At first I thought it was a butterfly that mimicked a moth, but I had never seen anything like it before. Next morning I got a glass jar half filled with bruised laurel leaves, and Ma got it in, and after a day or two I set it, clumsily, and meant to take it to London, but had no small box to put it in. I told Mr. Rothschild about it, and he said it sounded like a *Castnia*—curious South American moths very near to butterflies. So he got out the drawer with

¹The present Lord Rothschild.

them, but mine was not there; then he got another drawer half-empty, and there it was—only a coloured drawing, but exactly like. It had been described, but neither the Museum nor Mr. Rothschild had got it! I had had the orchids nearly a year and a half, so it must have been in the chrysalis all that time and longer, which Mr. Rothschild said was the case with the *Cast-nias*. On going home I searched, and found the brown chrysalis-case it had come out of among the roots of the same orchid the little *Longicornes* had dropped from. It is, I am pretty sure, a Brazilian species, and I have written to ask Mr. Hall if he knows where it came from. I have sent the moth and chrysalis to Prof. Poulton (I had promised it to him at the lecture) for the Oxford collection, and he is greatly pleased with it; and especially with its history—one quite small bit of an orchid, after more than a year in a greenhouse, producing a rare or new beetle and an equally rare moth! . . .

I am glad to say I feel really better than any time the last ten years.—A. R. W.

The Rev. O. Pickard-Cambridge has kindly written his reminiscence of another very curious coincidence connected with a natural history object.

“Some years ago, on looking over some insect drawers in my collection, Mr. A. R. Wallace exclaimed, ‘Why, there is my old Sarawak spider!’ ‘Well! that is curious,’ I replied, ‘because that spider has caused me much trouble and thought as to who might have caught it, and where; I had only lately decided to describe and figure it, even though I could give the name of neither locality nor finder, being, as it seemed to me, of a genus and species not as yet recorded; also I had, as you see, provisionally conferred your name upon it, although I had not the remotest idea that it had anything else to do with you.’ ‘Well,’ said Mr. Wallace, ‘if it is my old spider it ought to have my own private ticket on the pin underneath.’ ‘It has a ticket,’ I replied, ‘but it is unintelligible to me; the spider came to me among some other items by purchase at the sale of Mr. Wilson Saunders’ collections.’ ‘If it is mine,’ said Wallace (examining it), ‘the ticket should be so-and-so. And it is! I caught this spider at Sarawak, and specially noted its remarkable form. I remem-

ber it as if it were yesterday, and now I find it here, and you about to publish it as a new genus and species to which, in total ignorance of whence it came or who caught it, you have given my name!" Thus it stands, and '*Friula Wallacii*, Camb. (family Gasteracanthidæ), taken by Alfred Russel Wallace at Sarawak,' is the (unique as I believe) type specimen, in my collection."

—O. P. C.

Dr. Wallace was very fond of reading good novels, and usually spent an hour or two, before retiring to bed, with what he called a "good domestic story." One of his favourite authors was Marion Crawford. Poetry appealed to him very strongly, and he had a good memory for his favourite verses, especially for those he had learned in his youth. Amongst his books were over fifty volumes of poetry.

He liked to see friends or interesting visitors, but he was rather nervous with strangers until he became interested in what they had to say. He enjoyed witty conversation, and especially a good story well told. No one laughed more heartily than he when he was much amused, and he would slap his hands upon his knees with delight.

He was very accessible to anyone who might have something to say worth hearing, and he had a great many visitors, especially during the last ten years of his life. Many people distinguished in science, literature, or politics called upon him, and he always enjoyed these visits and the excitement of them seemed to have no bad effects upon him, even in the last year, when we sometimes feared he might be fatigued by them. In consequence of his sympathy with many heterodox ideas he frequently had visits from "cranks" who wished to secure his support for some new theory or "discovery." He would listen patiently, perhaps ask a few questions, and then endeavour to point out their fallacies. He would amuse us afterwards by describing their "preposterous ideas," and if much bored, he would speak of them as "muffs." He was loath to hurt their feelings, but he generally ended by expressing his opinion quite clearly, occasionally to their discomfort.

Dr. Littledale has contributed some reminiscences which may be introduced here.

"When I first met Dr. Wallace the conversation turned on the types of visitors that came to see him, and he gave us an amusing account of two young women who called on him to read through a most ponderous treatise relating to the Universe (I think it was). At all events the treatise proved, amongst other things, that Kepler's laws were all wrong. Dr. Wallace was very busy at the time, and politely declined to undertake the task. I remember him well describing with his hands the size of this enormous manuscript and laughing heartily as he detailed how the writer of the manuscript, the elder of the two sisters, persistently tried to persuade him that her theories were all absolutely proved in the work, while the younger sister acted as a sort of echo to her sister. The climax came in a fit of weeping, and as Dr. Wallace described it, the whole fabric of the universe was washed away in a flood of tears.

"On one occasion, when I was asked by Mrs. Wallace to see Dr. Wallace professionally, he was lying on the sofa in his study by the fire wrapped up in rugs, having just got over a bad shivering attack or rigor. His temperature was 104° Fahr., and all the other usual signs of acute fever were present, but nothing to enable one to form a positive opinion as to the cause. It must have been forty years since he had been in the tropics, but I think he felt that it was an attack of malarial fever. Knowing my patient, my treatment consisted in asking what he was going to do for himself. 'Well,' he said, 'I am going to have a hot bath and then go to bed, and to-morrow I shall get up and go into the garden as usual.' And he was out in the garden next day when I went to see him. This was an instance, doubtless one of many, of the 'will to live,' which carried him through a long life.

"Once, when he was talking about the gaps in the evolution of life, viz. between the inorganic and organic, between vegetable and animal, and between animal and man, I asked, 'Why postulate a beginning at all? We are satisfied with illimitability at one end, why not at the other?' 'For the simple reason,' he said, 'that the mind cannot comprehend anything that has never had a beginning.'

"What attracted me to him most, I think, was his remarkable simplicity of language, whatever the topic of conversation might be, and this not the simplicity of the great mind bringing itself

down to the level of the ordinary individual, but his customary mode of expression. I have heard him say that he felt the need of the fluency of speech which Huxley possessed, as he had to cast about for the expression that he wanted. This may have been the case when he was lecturing, but I certainly never noticed it in conversation."—H. E. L.

Dr. Wallace was always interested in young men and others who were going abroad with the intention of studying Natural History, and gave them what advice and help he could. He much enjoyed listening to the accounts given by travellers of the scenes, animals and plants and native life they had seen, and deplored the so-called civilising of the natives, which, in his opinion, generally meant their exploitation by Europeans, leading to their deterioration and extermination.

His nervousness with strangers sometimes led them to form quite erroneous impressions. It occasionally found expression in a nervous laugh which had nothing to do with amusement or humour, but was often heard when he was most serious and felt most deeply. One or two interviewers described it as a "chuckle," an expression which suggested feelings most opposite to those which he really experienced.

Although he could draw and sketch well, he did not take much pleasure in it, and only exercised his skill when there was a definite object in view. His sketches show a very delicate touch, and denote painstaking accuracy, while some are quite artistic. He much preferred drawing with compasses and squares, there being a practical object in his mind for which the plans or drawings were only the first steps. Even in his ninety-first year he found much enjoyment in drawing plans, and spent many hours in designing alterations to a small cottage which his daughter had bought.

He was interested in literary puzzles and humorous stories, and he preserved in an old scrap-book any that appealed to him. He would sometimes read some of them on festive occasions, or when we had children's parties, and sometimes he laughed so heartily himself that he could not go on reading.

In reviewing the years during which Dr. Wallace lived at Broadstone, the last decade, when he was between eighty and ninety years of age, this period seems to have been one of the

most eventful, and as full of work and mental activity as any previous period. He never tired of his garden, in which he succeeded in growing a number of rare and curious shrubs and plants. Our mother shared his delight and interest in the garden, and knew a great deal about flowers. She had an excellent memory for their botanical names, and he often asked her the name of some plant which he was pointing out to a friend and which for the moment he had forgotten. She was very fond of roses and of primroses, and there was a fine display of these flowers at "Old Orchard." She was successful in "budding" and hybridising roses, and produced several beautiful varieties. She was proficient in raising seeds, and he sometimes placed some which he received from abroad in her charge.

When he first came to live at Broadstone he frequently took short walks to the post or to the bank, and sometimes went by train to Poole on business, but he gradually went out less and less, till in the last few years he seldom went outside the garden, but strolled about looking at the flowers or supervising the construction of a new bed or rockery. During his last years his gardener wheeled him about the garden in a bath-chair when he did not feel strong enough to walk all the time.

In 1913, after his last two small books were written, he did no more writing except correspondence. This he attended to himself, except on one or two occasions when he was not very well or felt tired, when he asked one of us to answer a few letters for him. He took great interest in a small cottage which had recently been acquired on the Purbeck Hills near the sea, and in September, much against our wishes, he went there for two nights, taking the gardener to look after him. Luckily the weather was fine, and the change and excitement seemed to do him good, and during the next month he was very bright and cheerful, though, as some of his letters to his old friend Dr. Richard Norris and to Dr. Littledale show, he had been becoming increasingly weak.

TO MISS NORRIS

Old Orchard, Broadstone, Dorset. December 10, 1912.

My dear Miss Norris,—I am very sorry to hear that your father is so poorly. The weather is terribly gloomy, and I have

not been outside my rooms and greenhouse for more than an hour a week perhaps, for the last two months, and feel the better for it. Just now I feel better than I have done for a year past, having at last, I think, hit upon a proper diet, though I find it very difficult to avoid eating or drinking too much of what I like best. . . . It is one of my fads that I hate to waste anything, and it is that partly which makes it so difficult for me to avoid overeating. From a boy I was taught to leave no scraps on my plate, and from this excellent general rule of conduct I now suffer in my old age! . . .—Yours very sincerely,

ALFRED R. WALLACE.

TO DR. LITTLEDALE

Old Orchard, Broadstone, Dorset. January 11, 1913.

Dear Dr. Littledale,—Many thanks for your kind congratulations and good wishes.¹ I am glad to say I feel still able to jog on a few years longer in this *very good* world—for those who can make the best of it.

I am now suffering most from “eczema,” which has settled in my legs, so that I cannot stand or walk for any length of time. Perhaps that is an outlet for something worse, as I still enjoy my meals, and usually feel as well as ever, though I have to be very careful as to *what* I eat.—With best wishes for your prosperity, yours very truly,

ALFRED R. WALLACE.

TO DR. NORRIS

Old Orchard, Broadstone, Dorset. October 4, 1913.

My dear Dr. Norris,—Except for a continuous weakness I seem improving a little in general health, and the chronic rheumatic pain in my right shoulder has almost passed away in the last month (after about three years), and I can impute it to nothing but about a quarter of a pint a day of Bulmer's Cider! A most agreeable medicine!

The irritability of the skin, however, continues, though the inflammation of the legs has somewhat diminished. . . .

¹ On his ninetieth birthday.

My increasing weakness is now my most serious trouble, as it prevents me really from doing any more work, and causes a large want of balance, and liability to fall down. Even moving about the room after books, etc., dressing and undressing, make me want to lie down and rest. . . .

With kind remembrances to your daughter, believe me yours
very sincerely, ALFRED. R. WALLACE.

In disposition Dr. Wallace was cheerful, and very optimistic, and remarkably even-tempered. If irritated he quickly recovered, and soon forgot all about the annoyance, but he was always strongly indignant at any injustice to the weak or helpless. When worried by business difficulties or losses he very soon recovered his optimism, and seemed quite confident that all would come right (as indeed it generally did), and latterly he became convinced that all his past troubles were really blessings in disguise, without which as a stimulant he would have done no useful work.

His life was a happy one, and even the discomforts caused by his ailments, which were at times very acute for days together, never prevented him from enjoying the contemplation of his flowers, nor disturbed the serenity of his temper, nor caused him to complain.

Although rather delicate all his life, he rarely stayed in bed; in fact, only once in our memory, during an illness at Parkstone, did he do so, and then only for one day.

On Saturday, November 1st (1913), he walked round the garden, and on the following day seemed very bright, and enjoyed his dinner and supper, but about nine o'clock he felt faint and shivered violently. We called in Dr. Norman, who came in about an hour, and we heard them having a long talk and even laughing, in the study. As the doctor left he said, "Wonderful man! he knows so much. I can do nothing for him."

The next day he did not get up at the usual time, but we felt no anxiety until noon, when he still showed no inclination to rise. He appeared to be dozing, and said he wanted nothing. From that time he gradually sank into semi-consciousness, and at half-past nine in the morning of Friday, November 7th, quietly passed on to that other life in which he was such a firm believer.

PART V

Social and Political Views

“When a country is well governed, poverty and a mean condition are things to be ashamed of. When a country is ill governed, riches and honour are things to be ashamed of.”—CONFUCIUS.

IN the above sentences, written long before the dawn of Christian civilisation, we have an apt summary of the social and political views of Alfred Russel Wallace.

As we have stated in a previous chapter, it was during his short stay in London as a boy, when he was led to study the writings and methods of Robert Owen, of New Lanark, that his mind first opened to the consideration of the inequalities of our social life.

During the six years which he spent in land-surveying he obtained a more practical knowledge of the laws pertaining to public and private property as they affected the lives and habits of both squire and peasant.

The village inn, or public house, was then the only place where men could meet to discuss topics of mutual interest, and it was there that young Wallace and his brother spent some of their own leisure hours listening to and conversing with the village rustics. The conversation was not ordinarily of an educational character, but occasionally experienced farmers would discuss agricultural and land problems which were beginning to interest Wallace.

In reading his books and essays written more than seventy years later, we are struck with the exceptional opportunities which he had of comparing social conditions, and commercial and individual prosperity during that long period, and of witnessing the introduction of many inventions. He used to enjoy recalling many of the discussions between intelligent mechanics

which he heard of in his early days regarding the introduction of the steam-engine. One and another declared that the grip of the engine on the rails would not be sufficient to draw heavy trucks or carriages; that the wheels, in fact, would whiz round instead of going on, and that it would be necessary to sprinkle sand in front of the wheels, or make the tyres rough like files. About this time, too, there arose a keen debate upon the relative merits of the new railroads and the old canals. Many thought that the former could never compete with the latter in carrying heavy goods; but facts soon proved otherwise, for in one district alone the traffic of the canal, within two years of the coming of the railway, decreased by 1,000,000 tons.

It was during these years, and when he and his brother were making a survey for the enclosure of some common lands near Llandrindod Wells, that Wallace finally became aware of the injustice towards the labouring classes of the General Enclosure Act.

In this particular locality the land to be enclosed consisted of a large extent of moor and mountain which, with other common rights, had for many years enabled the occupants of the scattered cottages around to keep a horse, cow, or a few sheep, and thus make a fairly comfortable living. Under the Act, the whole of this open land was divided among the adjacent landowners of the parish or manor, in proportion to the size or value of their estates. Thus, to those who actually possessed much, much was given; whilst to those who only nominally owned a little land, even that was taken away in return for a small compensation which was by no means as valuable to them as the right to graze their cattle. In spite of the statement set forth in the General Enclosure Act—"Whereas it is expedient to facilitate the enclosure and improvement of common and other lands now subject to the rights of property which obstruct cultivation and the productive employment of labour," Wallace ascertained many years later that no single part of the land so enclosed had been cultivated by those to whom it was given, though certain portions had been let or sold at fabulous prices for building purposes, to accommodate summer visitors to the neighbourhood. Thus the unfortunate people who had formerly enjoyed home, health, and comparative prosperity in the cottages scattered over this common land had been obliged to migrate to the large towns,

seeking for fresh employment and means of subsistence, or had become "law-created paupers"; whilst to crown all, the piece of common originally "reserved" for the benefit of the inhabitants had been turned into golf-links!

Again and again Wallace drew attention to the fundamental duties of land ownership, maintaining that the public, as a whole, had become so blinded by custom that no effectual social reform would ever be established unless some strenuous and unrelenting effort was made to recover the land by law from those who had made the land laws and who had filched the common heritage of humanity for their own private aggrandisement.

With regard to the actual value of land, Wallace pointed out that the last valuation was made in the year 1692, and therefore, with the increase of value through minerals and other products since then, the arrears of land tax due up to 1905 would amount to more than the value of all the agricultural land of our country at the present time; therefore existing landlords, in clamouring for their alleged rights of property, might find out that those "rights" no longer exist.

Yet another point on which he insisted was the right of way through fields or woodlands, and especially beside the sea. With the advent of the motor-car and other swift means of locomotion, the public roads are no longer safe and pleasurable for pedestrians; besides the iniquitous fact that hundreds are kept from enjoying the beauties of nature by the utterly selfish and useless reservations of such bypaths by the landowner.

"This all-embracing system of land-robbery," again he writes, "for which nothing is too great or too small; which has absorbed meadow and forest, moor and mountain, which has appropriated most of our rivers and lakes and the fish that live in them; making the agriculturist pay for his seaweed manure and the fisherman for his bait of shell-fish; which has desolated whole counties to replace men by sheep or cattle, and has destroyed fields and cottages to make a wilderness for deer and grouse; which has stolen the commons and filched the roadside wastes; which has driven the labouring poor into the cities, and thus been the chief cause of the misery, disease, and early death of thousands . . . it is the advocates of this inhuman system who, when a partial restitution of their unholy gains is proposed, are the loudest in their cries of 'robbery'!"

"But all the robbery, all the spoliation, all the legal and illegal filching, has been on *their* side. . . . They made the laws to legalise their actions, and, some day, we, the people, will make laws which will not only legalise but justify our process of restitution. It will justify it, because, unlike their laws, which always took from the poor to give to the rich—to the very class which made the laws—ours will only take from the superfluity of the rich, *not* give to the poor or to any individuals, but to so administer as to enable every man to live by honest work, to restore to the whole people their birthright in their native soil, and to relieve all alike from a heavy burden of unnecessary and unjust taxation. *This* will be the true statesmanship of the future, and it will be justified alike by equity, by ethics, and by religion."

These, then, are the facts and reasons upon which Dr. Wallace based his strenuous advocacy of Land Nationalisation.¹ It was only by slow degrees that he arrived at some of the conclusions propounded in his later years, but once having grasped their full importance to the social and moral well-being of the community, he held them to the last.

The first book which tended to fasten his attention upon these matters was "Social Statics," by Herbert Spencer, but in 1870 the publication of his "Malay Archipelago" brought him into personal contact with John Stuart Mill, through whose invitation he became a member of the General Committee of the Land Tenure Reform Association. On the formation of the Land Nationalisation Society in 1880 he retired from the Association, and devoted himself to the larger issues which the new Society embraced.

Soon after the latter Society was started, Henry George, the American author of "Progress and Poverty," came to England, and Wallace had many opportunities of hearing him speak in public and of discussing matters of common interest in private. In spite of the ridicule poured upon Henry George's book by many eminent social reformers, Wallace consistently upheld its general principles.

His second work on these various subjects was a small book entitled "Bad Times," issued in 1885, in which he went deeply into the root causes of the depression in trade which had lasted

¹ See his book, "Land Nationalisation, its Necessity and its Aims" (1882).

since 1874. The facts there given were enlarged upon and continually brought up to date in his later writings. Articles which had appeared in various magazines were gathered together and included, with those on other subjects, in "Studies, Scientific and Social." His last three books, which include his ideas on social diseases and the best method of preventing them, were "The Wonderful Century," "Social Environment and Moral Progress," and "The Revolt of Democracy"; the two last being issued, as we have seen, in 1913, the year of his death.

In "Social Environment and Moral Progress" the conclusion of his vehement survey of our moral and social conditions was startling: *"It is not too much to say that our whole system of Society is rotten from top to bottom, and that the social environment as a whole in relation to our possibilities and our claims is the worst that the world has ever seen."*

That terrible indictment was doubly underscored in his MS.

What, in his mature judgment, were the causes and remedies? He set them out in this order:

1. The evils are due, broadly and generally, to our living under a system of universal competition for the means of existence, the remedy for which is equally universal co-operation.

2. It may also be defined as a system of economic antagonism, as of enemies, the remedy being a system of economic brotherhood, as of a great family, or of friends.

3. Our system is also one of monopoly by a few of all the means of existence—the land, without access to which no life is possible; and capital, or the results of stored-up labour, which is now in the possession of a limited number of capitalists, and therefore is also a monopoly. The remedy is freedom of access to land and capital for all.

4. Also, it may be defined as social injustice, inasmuch as the few in each generation are allowed to inherit the stored-up wealth of all preceding generations, while the many inherit nothing. The remedy is to adopt the principle of equality of opportunity for all, or of universal *inheritance by the state in trust for the whole community*.

"We have," he finally concluded, "ourselves created an immoral or unmoral social environment. To undo its inevitable results we must reverse our course. We must see that *all* our economic legislation, *all* our social reforms, are in the very oppo-

site direction to those hitherto adopted, and that they tend in the direction of one or other of the four fundamental remedies I have suggested. In this way only can we hope to change our existing immoral environment into a moral one, and *initiate a new era of Moral Progress.*"

The "Revolt of Democracy"¹ was addressed directly to the Labour Party. And once again he drew a vivid picture of how, during the whole of the nineteenth century, there was a continuous advance in the application of scientific discovery to the arts, especially to the invention and application of labour-saving machinery; and how our wealth had increased to an equally marvellous extent.

He pointed out that various estimates which had been made of the increase in our wealth-producing capacity showed that, roughly speaking, the use of mechanical power had increased it more than a hundredfold during the century; yet the result had been to create a limited upper class, living in unexampled luxury, while about one-fourth of the whole population existed in a state of fluctuating penury, often sinking below the margin of poverty. Many thousands were annually drawn into this gulf of destitution, and died from direct starvation and premature exhaustion or from diseases produced by unhealthy employment.

During this long period, however, although wealth and want had alike increased side by side, public opinion had not been sufficiently educated to permit of any effectual remedy being applied. The workers themselves had failed to visualise its fundamental causes, land monopoly and the competitive system of industry giving rise to an ever-increasing private capitalism which, to a very large extent, had controlled the Legislature. All through the last century this rapid accumulation of wealth due to extensive manufacturing industries led to a still greater increase of middlemen engaged in the distribution of the products, from the wealthy merchant to the various grades of tradesmen and small shopkeepers who supplied the daily wants of the community.

To those who lived in the midst of this vast industrial system, or were a part of it, it seemed natural and inevitable that there

¹Although this book was his last published work, it was written before "Social Environment and Moral Progress." He handed me the MS. a few months before his death.—The Editor.

should be rich and poor; and this belief was enforced on the one hand by the clergy, and on the other by political economists, so that religion and science agreed in upholding the competitive and capitalistic system of society as the only rational and possible one. Hence it came to be believed that the true sphere of governmental action did not include the abolition of poverty. It was even declared that poverty was due to economic causes over which governments had no power; that wages were kept down by the "iron law" of supply and demand; and that any attempt to find a remedy by Acts of Parliament only aggravated the disease. During the Premiership of Sir Henry Campbell-Bannerman this attitude was, for the first time, changed. On numerous occasions Sir Henry declared that he held it to be the duty of a government to deal with problems of unemployment and poverty.

In 1908 three great strikes, coming in rapid succession—those of the Railway and other Transport Unions, the Miners, and the London Dock Labourers—brought home to the middle and upper classes, and to the Government, how completely all are dependent on the "working classes." This and similar experiences showed us that when the organisation of the trade unions was more complete, and the accumulated funds of several years were devoted to this purpose, the bulk of the inhabitants of London, and of other great cities, could be made to suffer a degree of famine comparable with that of Paris when besieged by the German army in 1870.

Wallace's watchword throughout these social agitations was "Equality of Opportunity for All," and the ideal method by which he hoped to achieve this end was a system of industrial colonisation in our own country whereby *all* would have a fair, if not an absolutely equal, share in the benefits arising from the production of their own labour, whether physical or mental.¹

With regard to the education of the people, especially as a stepping-stone to moral and intellectual reform, Wallace believed in the training of individual natural talent, rather than the present system of general education thrust upon every boy or girl regardless of their varying mental capacities. He also urged that the building-up of the mind should be alternated with

¹ A full account of this scheme is given in his "Studies, Scientific and Social," Chap. xxvi.

physical training in one or more useful trades, so that there might be, not only at the outset, but also in later life, a choice of occupation in order to avoid the excess of unemployment in any one direction.

In his opinion, one of the injurious results of our competitive system, having its roots, however, in the valuable "guilds" of a past epoch, was the almost universal restriction of our workers to only one kind of labour. The result was a dreadful monotony in almost all spheres of work, the extreme unhealthiness of many, and a much larger amount of unemployment than if each man or woman were regularly trained in two or more occupations. In addition to two of what are commonly called trades, every youth should be trained for one day a week or one week in a month, according to the demand for labour, in some of the various operations of farming or gardening. Not only would this improve the general health of the workers, but it would also add much to the interest and enjoyment of their lives.

"There is one point," he wrote, "in connection with this problem which I do not think has ever been much considered or discussed. It is the undoubted benefit to all the members of a society of *the greatest possible diversity of character*, as a means both towards the greatest enjoyment and interest of association, and to the highest ultimate development of the race. If we are to suppose that man might have been created or developed with none of those extremes of character which now often result in what we call wickedness, vice, or crime, there would certainly have been a greater monotony in human nature, which would, perhaps, have led to less beneficial results than the variety which actually exists may lead to. We are more and more getting to see that very much, perhaps all, the vice, crime, and misery that exists in the world is the result, not of the wickedness of individuals, but of the entire absence of sympathetic training from infancy onwards. So far as I have heard, the only example of the effects of such a training on a large scale was that initiated by Robert Owen at New Lanark, which, with most unpromising materials, produced such marvellous results on the character and conduct of the children as to seem almost incredible to the numerous persons who came to see and often critically to examine them. There must have been all kinds of characters in his schools, yet *none* were found to be incorrigible, *none* beyond control, *none*

who did not respond to the love and sympathetic instruction of their teachers. It is therefore quite possible that *all* the evil in the world is directly due to man, not to God, and that when we once realise this to its full extent we shall be able, not only to eliminate almost completely what we now term evil, but shall then clearly perceive that all those propensities and passions that under bad conditions of society inevitably led to it, will under good conditions add to the variety and the capacities of human nature, the enjoyment of life by all, and at the same time greatly increase the possibilities of development of the whole race. I myself feel confident that this is really the case, and that such considerations, when followed out to their ultimate issues, afford a complete solution of the great problem of the ages—the origin of evil.”¹

Closely allied with the welfare of the child is another “reform” with which Wallace’s name will long be associated. That is his strong denunciation of Vaccination. For seven years he laboured to show medical and scientific men that statistics proved beyond doubt the futility of this measure to prevent disease. A few were converted, but public opinion is hard to move.

In his ideal of the future, Dr. Wallace gave a large and honoured sphere to women. He considered that it was in the highest degree presumptuous and irrational to attempt to deal by compulsory enactments with the most vital and most sacred of all human relationships, regardless of the fact that our present phase of social development is not only extremely imperfect, but, as already shown, vicious and rotten to the core. How could it be possible to determine by legislation those relations of the sexes which shall be best alike for individuals and for the race in a society in which a large proportion of our women are forced to work long hours daily for the barest subsistence, with an almost total absence of the rational pleasures of life, for the want of which thousands are driven into uncongenial marriages in order to secure some amount of personal independence or physical well-being? He believed that when men and women are, for the first time in the course of civilisation, equally free to follow their best impulses; when idleness and vicious and hurtful luxury on the one hand, and oppressive labour and the dread of starva-

¹ “My Life,” ii. 237–8.

tion on the other, are alike unknown; when *all* receive the best and broadest education that the state of civilisation and knowledge will admit; when the standard of public opinion is set by the wisest and the best among us, and that standard is systematically inculcated in the young—then we shall find that a system of truly “Natural Selection” (a term that Wallace preferred to “Eugenics,” which he utterly disliked) will come spontaneously into action which will tend steadily to eliminate the lower, the less developed, or in any way defective types of men, and will thus continuously raise the physical, moral, and intellectual standard of the race.

He further held that “although many women now remain unmarried from necessity rather than from choice, there are always considerable numbers who feel no strong impulse to marriage, and accept husbands to secure subsistence and a home of their own rather than from personal affection or sexual emotion. In a state of society in which all women were economically independent, where all were fully occupied with public duties and social or intellectual pleasures, and had nothing to gain by marriage as regards material well-being or social position, it is highly probable that the numbers of unmarried from choice would increase. It would probably come to be considered a degradation for any woman to marry a man whom she could not love and esteem, and this reason would tend at least to delay marriage till a worthy and sympathetic partner was encountered.”

But this choice, he considered, would be further strengthened by the fact that, with the ever-increasing approach to equality of opportunity for every child born in our country, that terrible excess of male deaths, in boyhood and early manhood especially, due to various preventable causes, would disappear, and change the present majority of women to a majority of men. This would lead to a greater rivalry for wives, and give to women the power of rejecting all the lower types of character among their suitors.

“It will be their special duty so to mould public opinion, through home training and social influence, as to render the women of the future the regenerators of the entire human race.” He fully hoped and believed that they would prove equal to the high and responsible position which, in accordance with natural laws, they will be called upon to fulfil.

Mr. D. A. Wilson, who visited him in 1912, writes:

"He surprised me by saying he was a Socialist—one does not expect a man like him to label himself in any way. It appeared to be unconscious modesty, like a schoolboy's, which made him willing to be labelled; but no label could describe him, and his mental sweep was unlimited. Although in his ninetieth year, he seemed to be in his prime. There was no sign of age but physical weakness, and you had to make an effort at times to remember even that. His eye kindled as he spoke, and more than once he walked about and chuckled, like a schoolboy pleased.

"An earnest expression like Carlyle's came over his countenance as he reprobated the selfish, wild-cat competition which made life harder and more horrible to-day for a well-doing poor man in England than among the Malays or Burmese before they had any modern inventions. Co-operation was the upward road for humanity. Men grew out of beasthood by it, and by it civilisation began. Forgetting it, men retrograded, subsiding swiftly, so that there were many individuals among us to-day who were in body, mind, and character below the level of our barbarian ancestors or contemporary "savages," to say nothing of civilised Burmese or Malays. What he meant by socialism can be seen from his books. Nothing in them surprised me after our talk. His appreciation of Confucius, when I quoted some things of the Chinese sage's which confirmed what he was saying, was emphatic, and that and many other things showed that socialism to him implied the upward evolution of humanity. It was because of the degradation of men involved that he objected to letting individuals grab the public property—earth, air and water. Monopolies, he thought, should at once revert to the public, and we had an argument which showed that he had no objection to even artificial monopolies if they were public property. He defended the old Dutch Government monopolies of spices, and declared them better than to-day's free trade, when cultivation is exploited by men who always tended to be mere money-grabbers, selfish savages let loose. In answer I mentioned the abuses of officialdom, as seen by me from the inside in Burma, and he agreed that the mental and moral superiority of many kinds of Asiatics to the Europeans who want to boss them made detailed European administration an absurdity. We should leave these peoples to develop in their own way. Having conquered Burma and India, he proceeded, the English should

take warning from history and restrict themselves to keeping the peace, and protecting the countries they had taken. They should give every province as much home rule as possible and as soon as possible, and study to avoid becoming parasites."

—D. A. W.

We may fittingly conclude this brief summary of Wallace's social views and ideals by citing his own reply to the question: "Why am I a Socialist?" "I am a Socialist because I believe that the highest law for mankind is justice. I therefore take for my motto, 'Fiat Justitia, Ruat Cœlum'; and my definition of Socialism is, 'The use, by everyone, of his faculties for the common good, and the voluntary organisation of labour for the equal benefit of all.' That is absolute social justice; that is ideal Socialism. It is, therefore, the guiding star for all true social reform."

He corresponded with Miss Buckley not only on scientific but also on public questions and social problems:

TO MISS BUCKLEY

Rosehill, Dorking. Sunday, [? December, 1878].

Dear Miss Buckley,— . . . How wonderfully the Russians have got on since you left! A very little more and the Turkish Government might be turned out of Europe—even now it might be with the greatest ease if our Government would join in giving them the last kick. Whatever power they retain in Europe will most certainly involve another war before twenty years are over.—Yours very faithfully,

ALFRED R. WALLACE.

TO MISS BUCKLEY

Waldron Edge, Croydon. May 2, 1879.

Dear Miss Buckley,— . . . My "Reciprocity" article seems to have produced a slight effect on the *Spectator*, though it did snub me at first, but it is perfectly sickening to read the stuff spoken and written, in Parliament and in all the newspapers, about the subject, all treating our present practice as something holy and immutable, whatever bad effects it may produce, and

though it is not in any way "free trade" and would I believe have been given up both by Adam Smith and Cobden.—Yours very faithfully,

ALFRED R. WALLACE.

He was always ready, even eager, to discuss his social and land nationalisation principles with his scientific friends, with members of his own family, and indeed with anyone who would lend a willing ear.

HERBERT SPENCER TO A. R. WALLACE

38 *Queen's Gardens, Bayswater, W.* April 25, 1881.

Dear Mr. Wallace,—As you may suppose, I fully sympathise with the general aims of your proposed Land Nationalisation Society; but for sundry reasons I hesitate to commit myself, at the present stage of the question, to a programme so definite as that which you send me. It seems to me that before formulating the idea in a specific shape it is needful to generate a body of public opinion on the general issue, and that it must be some time before there can be produced such recognition of the general principle involved as is needful before definite plans can be set forth to any purpose. . . . —Truly yours,

HERBERT SPENCER.

HERBERT SPENCER TO A. R. WALLACE

38 *Queen's Gardens, Bayswater, W.* July 6, 1881.

Dear Mr. Wallace,—I have already seen the work you name, "Progress and Poverty," having had a copy, or rather two copies, sent me. I gathered from what little I glanced at that I should fundamentally disagree with the writer, and have not read more.

I demur entirely to the supposition, which is implied in the book, that by any possible social arrangements whatever the distress which humanity has to suffer in the course of civilisation could have been prevented. The whole process, with all its horrors and tyrannies, and slaveries, and wars, and abominations of all kinds, has been an inevitable one accompanying the survival and spread of the strongest, and the consolidation of

small tribes into large societies; and among other things the lapse of land into private ownership has been, like the lapse of individuals into slavery, at one period of the process altogether indispensable. I do not in the least believe that from the primitive system of communistic ownership to a high and finished system of State ownership, such as we may look for in the future, there could be any transition without passing through such stages as we have seen and which exist now. Argument aside, however, I should be disinclined to commit myself to any scheme of immediate action, which, as I have indicated to you, I believe at present premature. For myself I feel that I have to consider not only what I may do on special questions, but also how the action I take on special questions may affect my general influence; and I am disinclined to give more handles against me than are needful. Already, as you will see by the enclosed circular, I am doing in the way of positive action more than may be altogether prudent.—Sincerely yours,

HERBERT SPENCER.

A. R. WALLACE TO MR. A. C. SWINTON

Frith Hill, Godalming. December 23, 1885.

My dear Swinton,— . . . I have just received an invitation to go to lecture in Sydney on Sundays for three months, with an intimation that other lectures can be arranged for in Melbourne and New Zealand. It is tempting! . . . If I had the prospect of clearing £1,000 by a lecturing campaign I would go, though it would require a great effort. . . . I did not think it possible even to contemplate going so far again, but the chance of earning a lot of money which would enable me to clear off this house and leave something for my family must be seriously considered.—Yours very truly,

ALFRED R. WALLACE.

TO MISS VIOLET WALLACE

Parkstone, Dorset. May 10, 1891.

My dear Violet,— . . . I am quite in favour of a legal eight hours' day. Overtime need not be forbidden, but every man who works overtime should have a legal claim to double wages

for the extra hours. That would make it cheaper for the master to employ two sets of men working each eight hours when they had long jobs requiring them, while for the necessities of finishing contracts, etc., they could well afford to pay double for the extra hours. "It would make everything dearer!" Of course it would! How else can you produce a more equal distribution of wealth than by making the rich and idle pay more and the workers receive more? "The workers would have to pay more, too, for everything they bought!" True again, but what they paid more would not equal their extra earnings, because a large portion of the extra pay to the men will be paid by the rich, and only the remainder paid by the men themselves. The eight hours' day and double pay for overtime would not only employ thousands now out of work, but would actually raise wages per hour and per day. This is clear, because wages are kept down wholly by the surplus supply of labour in every trade. The moment the surplus is used up, or nearly so, by more men being required on account of shorter hours, competition among the men becomes less; among the employers, for men, more: hence necessarily higher wages all round. As to the bogey of foreign competition, it is a bogey only. All the political economists agree that if wages are raised in all trades, it will not in the least affect our power to export goods as profitably as now. Look and see! And, secondly, the eight hours' movement is an international one, and will affect all alike in the end.

There are some arguments for you! Poor unreasoning infant!! . . .

REV. AUGUSTUS JESSOP TO A. R. WALLACE

Scarning Rectory, East Dereham. August 25, 1893.

My dear Mr. Wallace,—I have put off writing to thank you for your kind letter, and the book and pamphlets you were good enough to send me, because I hoped in acknowledgment to say I had read your little volumes, as I intend to. The fates have been against me, and I will delay no longer thanking you for sending them to me.

I do not believe in your theory of land nationalisation one bit! But I like to see all that such a man as you has to say on his side.

In return I send you my view of the matter, which is just as likely to convert you as your book is to convert me.

I love a man with a theory, for I learn most from such a man, and when I have thought a thing out in my own mind and forgotten the arguments while I have arrived at a firm conviction as to the conclusion, it is refreshing to be reminded of points and facts that have slipped away from me!

It was a great pleasure and privilege to make your acquaintance the other day, and I hope we may meet again some day.—
Very truly yours,

AUGUSTUS JESSOP.

REV. H. PRICE HUGHES TO A. R. WALLACE

8 Tavilton Street, Gordon Square, W.C. September 14, 1898.

Dear Dr. Wallace,—I am always very glad when I hear from you. So far as your intensely interesting volume has compelled some very prejudiced people to read your attack on modern delusions, it is a great gain, especially to themselves. I have read your tract on "Justice, not Charity," with great pleasure and approval. The moment Mr. Benjamin Kidd invented the striking term of "equality of opportunity" I adopted it, and have often preached it in the pulpit and on the platform, just as you preach it in the tract before me. I fully agree that justice, not charity, is the fundamental principle of social reform. There is something very contemptible in the spiteful way in which many newspapers and magistrates are trying to aggravate the difficulties of conscientious men who avail themselves of the conscience clause in the new Vaccination Act. There is very much to be done yet before social justice is realised, but the astonishing manifesto of the Czar of Russia, which I have no doubt is a perfectly sincere one, is a revelation of the extent to which social truth is leavening European society. Since I last wrote to you I have been elected President of the Wesleyan Methodist Conference, which will give me a great deal of special work and special opportunities also, I am thankful to say, of propagating Social Christianity, which in fact, and to a great extent in form, is what you yourself are doing.—Yours very sincerely,

H. PRICE HUGHES.

TO ALFRED RUSSELL

Parkstone, Dorset. May 11, 1900.

Dear Sir,—I am not a vegetarian, but I believe in it as certain to be adopted in the future, and as essential to a higher social and moral state of society. My reasons are:

(1) That far less land is needed to supply vegetable than to supply animal food.

(2) That the business of a butcher is, and would be, repulsive to all refined natures.

(3) That with proper arrangements for variety and good cookery, vegetable food is better for health of body and mind.

—Yours very truly,

ALFRED R. WALLACE.

TO MR. JOHN (LORD) MORLEY

Parkstone, Dorset. October 20, 1900.

Dear Sir,—I look upon you as the one politician left to us, who, by his ability and integrity, his eloquence and love of truth, his high standing as a thinker and writer, and his openness of mind, is able to become the leader of the English people in their struggle for freedom against the monopolists of land, capital, and political power. I therefore take the liberty of sending you herewith a book of mine containing a number of miscellaneous essays, a few of which, I venture to think, are worthy of your serious attention.

Some time since you intimated in one of your speeches that, if the choice for this country were between Imperialism and Socialism, you were inclined to consider the latter the less evil of the two. You added, I think, your conviction, that the dangers of Socialism to human character were what most influenced you against it. I trust that my impression of what you said is substantially correct. Now I myself believe, after a study of the subject extending over twenty years, that this danger is non-existent, and certainly does not in any way apply to the fundamental principles of Socialism, which is, simply, *the voluntary organisation of labour for the good of all*. . . . —With great esteem, I am, yours very faithfully,

ALFRED R. WALLACE.

MR. JOHN (LORD) MORLEY TO A. R. WALLACE

57 Elm Park Gardens, S.W. October 31, 1900.

My dear Sir,—For some reason, though your letter is dated the 20th, it has only reached me, along with the two volumes, to-day. I feel myself greatly indebted to you for both. In older days I often mused upon a passage of yours in the “Malay Archipelago” contrasting the condition of certain types of savage life with that of life in a modern industrial city. And I shall gladly turn again to the subject in these pages, new to me, where you come to close quarters with the problem.

But my time and my mind are at present neither of them free for the effective consideration of this mighty case. Nor can I promise myself the requisite leisure for at least several months to come. What I can do is to set your arguments a-simmering in my brain, and perhaps when the time of liberation arrives I may be in a state to make something of it. I don't suppose that I shall be a convert, but I always remember J. S. Mill's observation, after recapitulating the evils to be apprehended from Socialism, that he would face them in spite of all, if the only alternative to Socialism were our present state.—With sincere thanks and regard, believe me yours faithfully,

JOHN MORLEY.

TO MR. C. G. STUART-MENTEITH

Parkstone, Dorset. June 5, 1901.

Dear Sir,—I have no time to discuss your letter ¹ at any length. You seem to assume that we can say definitely who are the “fit” and who the “unfit.”

I deny this, except in the most extreme cases.

I believe that, even now, the race is mostly recruited by the *more fit*—that is the upper working classes, and the lower middle classes.

Both the very rich and the very poor are probably—as classes—below these. The former increase less rapidly through immorality and late marriage; the latter through excessive infant mortality. If that is the case, no legislative interference is needed, and would probably do harm.

¹ Advocating Eugenics and the segregation of the unfit.

I see nothing in your letter which is really opposed to my contention—that under rational social conditions the healthy instincts of men and women will solve the population problem far better than any tinkering interference either by law or by any other means.

And in the meantime the condition of things is not so bad as you suppose.—Yours very truly,

ALFRED R. WALLACE.

TO MR. SYDNEY COCKERELL

Broadstone, Wimborne. January 15, 1906.

Dear Mr. Cockerell,—I have now finished reading Kropotkin's *Life with very great interest*, especially for the light it throws on the present condition of Russia. It also brings out clearly some very fine aspects of the Russian character, and the horrible despotism to which they are still subject, equivalent to that of the days of the Bastille and the system of *Lettres de cachet* before the great Revolution in France. It seems to me probable that under happier conditions—perhaps in the not distant future—Russia may become the most advanced instead of the most backward in civilisation—a real leader among nations, not in war and conquest but in social reform.—Yours faithfully,

A. R. WALLACE.

TO MR. J. HYDER (OF THE LAND NATIONALISATION SOCIETY)

Broadstone, Wimborne. May 13, 1907.

Dear Mr. Hyder,—Although it is not safe to hallo before one is out of the wood, I think I may congratulate the Society upon the prospect it now has of obtaining the first fruits of its persistent efforts, for a quarter of a century, to form an enlightened public opinion in favour of our views. If the Government adequately fulfils its promises, we shall have, in the Bill for a fair valuation of land apart from improvements, as a basis of taxation and for purchase, and that giving local authorities full powers to acquire land so valued, the first real and definite steps towards complete nationalisation. . . .

ALFRED R. WALLACE.

TO MR. A. WILTSHIRE ¹*Broadstone, Wimborne. October 10, 1907.*

Dear Sir,—I told Mr. Button that I do not approve of the resolution you are going to move.²

The workers of England have themselves returned a large majority of ordinary Liberals, including hundreds of capitalists, landowners, manufacturers, and lawyers, with only a sprinkling of Radicals and Socialists. The Government—your own elected Government—is doing more for the workers than any Liberal Government ever did before, yet you are going to pass what is practically a vote of censure on it for not being a Radical, Labour, and Socialist Government!

If this Government attempted to do what you and I think ought to be done, it would lose half its followers and be turned out, ignominiously, giving the Tories another chance. That is foolish as well as unfair.—Yours truly,

ALFRED R. WALLACE.

TO LORD AVEBURY

Broadstone, Wimborne. June 23, 1908.

Dear Lord Avebury,— . . . Allow me to wish every success to your Bill for preserving beautiful birds from destruction. To stop the import is the only way—short of the still more drastic method of heavily fining everyone who wears feathers in public, with imprisonment for a second offence. But we are not yet ripe for that.—Yours very truly,

ALFRED R. WALLACE.

TO MR. E. SMEDLEY

Old Orchard, Broadstone, Dorset. December 25, 1910.

Dear Mr. Smedley,—Thanks for your long and interesting letter. . . . Man is, and has been, horribly cruel, and it is indeed difficult to explain why. Yet that there is an explanation, and that it does lead to good in the end, I believe. Praying is evi-

¹ Hon. Sec. of the Federated Trades and Labour Council, Bournemouth.

² At an Old Age Pension meeting.

dently useless, and should be, as it is almost always selfish—for *our* benefit, or *our families*, or *our nation*.—Yours very truly,
ALFRED R. WALLACE.

TO MR. W. G. WALLACE

Old Orchard, Broadstone, Wimborne. August 20, 1911.

My dear Will,— . . . The railway strike surpasses the Parliament Bill in excitement. On receipt of Friday's paper, I sat down and composed and sent off to Lloyd George a short but big letter, on large foolscap paper, urging him and Asquith, as the two strong men of the Government, to take over at once the management of the railways of the entire country, by Royal Proclamation—on the ground of mismanagement for seventy years, and having brought the country to the verge of starvation and civil war; to grant an amnesty to all strikers (except for acts of violence), also grant all the men's demands for one year, and devote that time to a deliberate and impartial inquiry and a complete scheme of reorganisation of the railways in the interest, first of the public, then of the men of all grades, lastly of the share and bond owners, who will become guaranteed public creditors. It has been admitted and proved again and again, that the men are badly treated, that their grievances are real—their very unanimity and standing by each other proves it. Their demands are most moderate; and the cost in extra wages will be saved over and over in safety, regularity, economy of working, and public convenience. I have not had even an acknowledgment of receipt yet, but hope to in a day or two. . . .

MR. H. M. HYNDMAN TO A. R. WALLACE

9 Queen Anne's Gate, Westminster, S.W. March 14, 1912.

Dear Sir,—Everyone who knows anything of the record of modern science in this country recognises how very much we all owe to you. It was, therefore, specially gratifying to me that you should be so kind as to write such a very encouraging letter on the occasion of my seventieth birthday. I owe you sincere thanks for what you said, though I may honestly feel that you overpraised what I have done. It has been an uphill fight, but I am lucky in being allowed to see through the smoke and dust

of battle a vision of the promised land. The transformation from capitalism to socialism is going on slowly under our eyes.

Again thanking you and wishing you every good wish, believe me yours sincerely,

H. M. HYNDMAN.

TO MR. M. J. MURPHY

Old Orchard, Broadstone, Dorset. August 19, 1913.

Dear Sir,—I not only think but firmly believe that Lloyd George is working for the good of the people, in all ways open to him. The wonder is that he can persuade Asquith and the Cabinet to let him go as far as he does. No doubt he is obliged to do things he does not think the best absolutely, but the best that are practicable. He does not profess to be a Socialist, and he is not infallible, but he does the best he can, under the conditions in which he finds himself. Socialists who condemn him for not doing more are most unfair. They must know, if they think, that if he tried to do much more towards Socialism he would break up the Government and let in the Tories.—Yours truly,

A. R. WALLACE.

TO MR. A. WILTSHIRE

Old Orchard, Broadstone, Dorset. September 14, 1913.

Dear Sir,—I wish you every success in your work for the amelioration of the condition of the workers, through whose exertions it may be truly said we all live and move and have our being.

Your motto is excellent. Above all things stick together.

Equally important is it to declare as a fixed principle that wages are to be and must be continuously raised, never lowered. You have too much arrears to make up—too many forces against you, to admit of their being ever lowered. Let future generations decide when that is necessary—if ever.

This is a principle worth enforcing by a general strike. Nothing less will be effective—nothing less should be accepted; and you must let the Government know it, and insist that they adopt it.

The rise must always be towards uniformity of payment for all useful and productive work.—Yours sincerely,

ALFRED R. WALLACE.

PART VI

Some Further Problems

I.—Astronomy

OF the varied subjects upon which Wallace wrote, none, perhaps, came with greater freshness to the general reader than his books written when he was nearly eighty upon the ancient science of astronomy.

Perhaps he would have said that the "directive Mind and Purpose" kept these subjects back until the closing years of his life in order that he might bring to bear upon them his wider knowledge of nature, enlightened by that spiritual perception which led him to link the heavens and the earth in one common bond of evolution, culminating in the development of moral and spiritual intelligences.

"Man's Place in the Universe" (1903) was in effect a prelude to "The World of Life" (1910). Wallace saw afterwards that one grew out of the other, as we find him frequently saying with regard to his other books and essays.

As with Spiritualism, so with Astronomy, the seed-interest practically lay dormant in his mind for many years; with this difference, however, that temperament and training caused a speedy unfolding of his mind when once a scientific subject gripped him, whereas with Spiritualism he felt the need of moving slowly and cautiously before fully accepting the phenomena as verifiable facts.

It was during the later period of his land surveying, when he was somewhere between the ages of 18 and 20, that he became distinctly interested in the stars. Being left much alone at this period, he began to vary his pursuits by studying a book on

Nautical Astronomy, and constructing a rude telescope.¹ This primitive appliance increased his interest in other astronomical instruments, and especially in the grand onward march of astronomical discovery, which he looked upon as one of the wonders of the nineteenth century.

It was the inclusion of astronomy in lectures he delivered at Davos which led him to extend his original brief notes into the four chapters which form an important part of his "Wonderful Century." He freely confessed that in order to write these chapters he was obliged to read widely, and to make much use of friends to whom astronomy was a more familiar study. And it was whilst he was engaged upon these chapters that his attention became riveted upon the unique position of our planet in relation to the solar system.

He had noticed that certain definite conditions appeared to be absolutely essential to the origin and development of the higher types of terrestrial life, and that most of these must have been certainly dependent on a very delicate balance of the forces concerned in the evolution of our planet. Our position in the solar system appeared to him to be peculiar and unique because, he thought, we may be almost sure that these conditions do not coexist on any other planet, and that we have no good reason to believe that other planets could have maintained over a period of millions of years the complex and equable conditions absolutely necessary to the existence of the higher forms of terrestrial life. Therefore it appeared to him to be proved that our earth does really stand alone in the solar system by reason of its special adaptation for the development of human life.

Granting this, however, the question might still be asked, Why should not any one of the suns in other parts of space possess planets as well adapted as our own to develop the higher forms of organic life? These questions cannot be answered definitely; but there are reasons, he considered, why the central position which we occupy may alone be suitable. It is almost certain that electricity and other mysterious radiant forces (of which we have so recently discovered the existence) have played an important part in the origin and development of organised life, and it does not appear to be extravagant to assume that the

¹ See p. 16.

extraordinary way in which these cosmic forces have remained hidden from us may be due to that central position which we are found to occupy in the whole universe of matter discoverable by us. Indeed, it may well be that these wonderful forces of the ether are more irregular—and perhaps more violent—in their effect upon matter in what may be termed the outer chambers of that universe, and that they are only so nicely balanced, so uniform in their action, and so concealed from us, as to be fit to aid in the development of organic life in that central portion of the stellar system which our globe occupies. Should these views as to the unique central position of our earth be supported by the results of further research, it will certainly rank as the most extraordinary and perhaps the most important of the many discoveries of the past century.

While still working on this section of his "Wonderful Century," he was asked to write a scientific article, upon any subject of his own choice, for the *New York Independent*. And as the idea of the unique position of the earth to be the abode of human life was fresh in his mind, he thought it would prove interesting to the general public. However, before his article appeared simultaneously in the American papers and in the *Fortnightly Review*, a friend who read it was so impressed with its originality and treatment that he persuaded Wallace to enlarge it into book form; and it appeared in the autumn of 1903 as "Man's Place in the Universe."

This fascinating treatise upon the position occupied by the earth, and man, in the universe, had the same effect as some of his former writings, of drawing forth unstinted commendation from many religious and secular papers; whilst the severely scientific and materialistic reviewers doubted how far his imagination had superseded unbiased reason.

On one point, however, most outsiders were in agreement—that he had invested an ancient subject with freshest interest through approaching it by an entirely new way. The plan followed was that of bringing together all the positive conclusions of the astronomer, the geologist, the physicist, and the biologist, and by weighing these carefully in the balance he arrived at what appeared to him to be the only reasonable conclusion. He therefore set out to solve the problem whether or not the logical inferences to be drawn from the various results of modern science

lent support to the view that our earth is the only inhabited planet, not only in our own solar system, but in the whole stellar universe. In the course of his close and careful exposition he takes the reader through the whole trend of modern scientific research, concluding with a summing-up of his deductions in the following six propositions, in the first three of which he sets out the conclusions reached by modern astronomers:

“(1) That the stellar universe forms one connected whole; and, though of enormous extent, is yet finite, and its extent determinable.

“(2) That the solar system is situated in the plane of the Milky Way, and not far removed from the centre of that plane. The earth is, therefore, nearly in the centre of the stellar universe.

“(3) That this universe consists throughout of the same kinds of matter, and is subjected to the same physical and chemical laws.

“The conclusions which I claim to have shown to have enormous probabilities in their favour are:

“(4) That no other planet in the solar system than our earth is inhabited or habitable.

“(5) That the probabilities are almost as great against any other sun possessing inhabited planets.

“(6) That the nearly central position of our sun is probably a permanent one, and has been specially favourable, perhaps absolutely essential, to life-development on the earth.”

Wallace never maintained that this earth alone in the whole universe is the abode of life. What he maintained was, first, that our solar system appears to be in or near the centre of the visible universe, and, secondly, that all the available evidence supports the idea of the extreme unlikelihood of there being on any star or planet revealed by the telescope any intelligent life either identical with or analogous to man. To suppose that this one particular type of universe extends over all space was, he considered, to have a low idea of the Creator and His power. Such a scheme would mean monotony instead of infinite variety, the key-note of things as they are known to us. There might be a million universes, but all different.

To his mind there was no difficulty in believing in the existence of consciousness apart from material organism; though he

could not readily conceive of pure mind, or pure spirit, apart from some kind of substantial envelope or substratum. Many of the views suggested in "Man's Place in the Universe" as to man's spiritual progress hereafter, the reason or ultimate purpose for which he was brought into existence, were enlarged upon, later, in "The World of Life." As early, however, as 1903, Wallace did not hesitate to express his own firm conviction that Science and Spiritualism were in many ways closely akin.

He believed that the near future would show the strong tendency of scientists to become more religious or spiritual. The process, he thought, would be slow, as the general attitude has never been more materialistic than now. A few have been bold enough to assert their belief in some outside power, but the leading scientific men are, as a rule, dead against them. "They seem," he once remarked, "to think, and to like to think, that the whole phenomena of life will one day be reduced to terms of matter and motion, and that every vegetable, animal, and human product will be explained, and may some day be artificially produced, by chemical action. But even if this were so, behind it all there would still remain an unexplained mystery."

Closely associated with "Man's Place in the Universe" is a small volume, "Is Mars Habitable?" This was first commenced as a review of Professor Percival Lowell's book, "Mars and its Canals," with the object of showing that the large amount of new and interesting facts contained in this work did not invalidate the conclusion that he (Wallace) had reached in 1903—that Mars is not habitable. The conclusions to which his argument led him were these:

"(1) All physicists are agreed that . . . Mars would have a mean temperature of about 35° F. owing to its distance from the sun.

"(2) But the very low temperatures on the earth under the equator at a height where the barometer stands at about three times as high as on Mars, proves that from scantiness of atmosphere alone Mars cannot possibly have a temperature as high as the freezing-point of water.

"The combination of these two results must bring down the temperature of Mars to a degree wholly incompatible with the existence of animal life.

“(3) The quite independent proof that water-vapour cannot exist on Mars, and that, therefore, the first essential of organic life—water—is non-existent.

“The conclusion from these three independent proofs . . . is therefore irresistible—that animal life, especially in its highest forms, cannot exist. Mars, therefore, is not only uninhabited by intelligent beings . . . but is absolutely uninhabitable.”

In contrast to his purely scientific interest in astronomy, Wallace was moved by the romance of the “stars,” akin to his enthusiastic love of beautiful butterflies. Had it not been for this touch of romance and idealism in his writings on astronomy, they would have lost much of their charm for the general reader. His breadth of vision transforms him from a mere student of astronomy into a seer who became ever more deeply conscious of the mystery both “before and behind.”

“Rain, sun, and rain! and the free blossom blows;
Sun, rain, and sun! and where is he who knows?
From the great deep to the great deep he goes.”

And whilst facing with brave and steady mind the great mysteries of earth and sky, of life and what lies beyond it, he himself loved to quote:

“Fear not thou the hidden purpose
Of that Power which alone is great,
Nor the myriad world His shadow,
Nor the silent Opener of the Gate.”

Among the scientific friends to whom he appealed for help when writing his astronomical books was Prof. (now Sir) W. F. Barrett.

TO PROF. BARRETT

Parkstone, Dorset. February 12, 1901.

My dear Barrett,—I shall be much obliged if you will give me your opinion on a problem in physics that I cannot find answered in any book. It relates to the old Nebular Hypothesis, and is this:

It is assumed that the matter of the solar system was once wholly gaseous, and extended as a roughly globular or lenticular mass beyond the orbit of Neptune. Sir Robert Ball stated in a lecture here that even when the solar nebula had shrunk to the size of the earth's orbit it must have been (I think he said) hundreds of times rarer than the residual gas in one of Crookes's high vacuum tubes. Yet, by hypothesis, it was hot enough, even in its outer portions, to retain all the solid elements in the gaseous state.

Now, admitting this to be *possible* at any given epoch, my difficulty is this: how long could the outer parts of this nebula exist, exposed to the zero temperature of surrounding space, without losing the gaseous state and aggregating into minute solid particles—into meteoric dust, in fact?

Could it exist an hour? a day? a year? a century? Yet the process of condensation from the Neptunian era to that of Saturn or Jupiter must surely have occupied millions of centuries. What kept the almost infinitely rare metallic gases in the gaseous state all this time? Is such a condition of things physically possible?

I cannot myself imagine any such condition of things as the supposed primitive solar nebula as possibly coming into existence under any conceivably antecedent conditions, but, granted that it did come into existence, it seems to me that the gaseous state must almost instantly begin changing into the solid state. Hence I adopt the meteoric theory instead of the nebular; since all the evidence is in favour of solid matter being abundant all through known space, while there is no evidence of metallic gases existing in space, except as the result of collisions of huge masses of matter. Is my difficulty a mare's nest?—Yours very truly,

ALFRED R. WALLACE.

TO MRS. FISHER

Broadstone, Wimborne. February 28, 1905.

Dear Mrs. Fisher,—Thanks for your letter. Am sorry I have not converted you, but perhaps it will come yet! I will only make one remark as to your conclusion.

I have not attempted to prove a negative! That is not necessary. What I claim to have done is, to have shown that all the

evidence we have, be it much or little, is decidedly against not only other solar-planets having inhabitants, but also, as far as probabilities are concerned, equally against it in any supposed stellar-planets—for not one has been proved to exist. There is absolutely no evidence which shows even a probability of there being other inhabited worlds. It is all pure speculation, depending upon our ideas as to what the universe is for, as to what *we* think (some of us!) *ought* to be! That is not evidence, even of the flimsiest. All I maintain is that mine *is* evidence, founded on physical probabilities, and that, as against no evidence at all—no proved physical probability—mine holds the field!—Yours very truly,

ALFRED R. WALLACE.

TO MR. E. SMEDLEY

Broadstone, Dorset. July 24, 1907.

Dear Mr. Smedley,— . . . I write chiefly to tell you that I have read Mr. Lowell's last book, "Mars and its Canals," and am now writing an article, or perhaps a small book, about it. I am sure his theories are all wrong, and I am showing why, so that anyone can see his fallacies. His observations, drawings, photographs, etc., are all quite right, and I believe true to nature, but his interpretation of what he sees is wrong—often even to absurdity. He began by thinking the straight lines are works of art, and as he finds more and more of these straight lines, he thinks that proves more completely that they are works of art, and then he twists all other evidence to suit that. The book is not very well written, but no doubt the newspaper men think that as he is such a great astronomer he must know what it all means!

I am more than ever convinced that Mars is totally uninhabitable. . . . —Yours very truly,

ALFRED R. WALLACE.

TO PROF. BARRETT

Broadstone, Wimborne. August 10, 1907.

My dear Barrett,—Thanks for your letter, and your friend Prof. Stroud's. I have come to the sad conclusion that it is

hopeless to get any mathematician to trouble himself to track out Lowell's obscurities and fallacies. . . .

So, being driven on to my own resources, I have worked out a mode of estimating (within limits) the temperature of Mars, without any mathematical formulæ—and only a little arithmetic. I want to know if there is any fallacy in it, and therefore take the liberty of sending it to you, as you are taking your holiday, just to read it over and tell me if you see any flaw in it. I also send my short summary of Lowell's *Philosophical Magazine* paper, so that you can see if my criticism at the end is fair, and whether his words really mean what to me they seem to. . . .

—Yours very sincerely,

ALFRED R. WALLACE.

TO MR. F. BIRCH

Sept. 12, 1907.

Dear Fred,— . . . For the last two or three months I have had a hard struggle with Mars—not the god of war, but the planet—writing a small book, chiefly criticising Lowell's last book, called "Mars and its Canals," published less than a year back by Macmillan, who will also publish my reply. I think it is crushing, but it has cost me a deal of trouble, as Lowell has also printed a long and complex mathematical article trying to prove that though Mars receives less than half the sun-heat we do, yet it is very nearly as warm and quite habitable! But his figures and arguments are alike so shaky and involved that I cannot get any of my mathematical friends to tackle it or point out his errors. However, I think I have done it myself by the rules of common sense. . . . —Your sincere friend,

ALFRED R. WALLACE.

TO MR. H. JAMYN BROOKE

Old Orchard, Broadstone, Wimborne. December 2, 1910.

Dear Sir,—Your "monistic" system is to me a system of mere contradictory words. You begin with three things—then you say they are correlated with one substance—co-extensive with the universe. This you cannot possibly know, and it is about as intelligible and as likely to be true as the Athanasian Creed!—

Yours truly,

ALFRED R. WALLACE.

TO PROF. KNIGHT

Old Orchard, Broadstone, Dorset. October 1, 1913.

Dear Mr. Knight,—I have written hardly anything on the direct proofs of "immortality" except in my book on "Miracles and Modern Spiritualism," and also in "My Life," Vol. II. But my two works, "Man's Place in the Universe" (now published at 1s.), and my later volume, "The World of Life," form together a very elaborate, and I think conclusive, scientific argument in favour of the view that the whole material universe exists and is designed for the production of Immortal Spirits, in the greatest possible diversity of nature, and character, corresponding with . . . the almost infinite diversity of that universe, in all its parts and in every detail. . . . —Yours very truly,

ALFRED R. WALLACE.

P.S.—I am fairly well, but almost past work.—A. R. W.

TO SIR OLIVER LODGE

Old Orchard, Broadstone, Dorset. October 9, 1913.

Dear Sir Oliver Lodge,—Owing to ill-health and other causes I have only now been able to finish the perusal of your intensely interesting and instructive Address to the British Association. I cannot, however, refrain from writing to you to express my admiration of it, and especially of the first half of it, in which you discuss the almost infinite variety and complexity of the physical problems involved in the great principle of "continuity" in so clear a manner that outsiders like myself are able to some extent to apprehend them. I am especially pleased to find that you uphold the actual existence and *continuity* of the ether as scientifically established, and reject the doubts of some mathematicians as to the reality and perfect continuity of space and time as unthinkable.

The latter part of the Address is even more important, and is especially notable for your clear and positive statements as to the evidence in all life-process of a "guiding" Mind. I can hardly suppose that you can have found time to read my rather discursive and laboured volume on "The World of Life," written mainly for the purpose of enforcing not only the proofs of a

"guiding" but also of a "foreseeing" and "designing" Mind by evidence which will be thought by most men of science to be unduly strained. It is, therefore, the more interesting to me to find that you have yourself (on pp. 33-34 of your Address) used the very same form of analogical illustration as I have done (at p. 296 of "The World of Life") under the heading of "A Physiological Allegory," as being a very close representation of what really occurs in nature.

To conclude: your last paragraph rises to a height of grandeur and eloquence to which I cannot attain, but which excites my highest admiration.

Should you have a separate copy to spare of your Romanes Lecture at Oxford, I should be glad to have it to refer to.—Believe me yours very truly,

ALFRED R. WALLACE.

The last of Wallace's letters on astronomical subjects was written to Sir Oliver Lodge about a week before his death:

TO SIR OLIVER LODGE

Old Orchard, Broadstone, Dorset. October 27, 1913.

Dear Sir Oliver Lodge,—Many thanks for your Romanes Lecture, which, owing to my ignorance of modern electrical theory and experiments, is more difficult for me than was your British Association Address.

I have been very much interested the last month by reading a book sent me from America by Mr. W. L. Webb, being "An Account of the Unparalleled Discoveries of Mr. T. J. J. See."

Several of Mr. See's own lectures are given, with references to his "Researches on the Evolution of the Stellar Systems," in two large volumes.

His theory of "capture" of suns, planets, and satellites seems to me very beautifully worked out under the influence of gravitation and a resisting medium of cosmical dust—which explains the origin and motions of the moon as well as that of all the planets and satellites far better than Sir G. Darwin's Expulsion theory.

I note however that he is quite ignorant that Proctor, forty years ago, gave full reasons for this "capture" theory in his

"Expanse of Heaven," and also that the same writer showed that the Milky Way could not have the enormous lateral extension he gives to it, but that it cannot really be much flattened. He does not even mention the proofs given of this both by Proctor and, I think, by Herbert Spencer, while in Mr. Webb's volume (opposite p. 212) is a diagram showing the "Coal Sack" as a "vacant lane" running quite through and across the successive spiral extensions laterally of the galaxy, without any reference or a word of explanation that such features, of which there are many, really demonstrate the untenability of such extension.

An even more original and extremely interesting part of Mr. See's work is his very satisfactory solution of the hitherto unsolved geological problem of the origin of all the great mountain ranges of the world, in Chapters X., XI., and XII. of Mr. Webb's volume. It seems quite complete except for the beginnings, but I suppose it is a result of the formation of the *earth* by accretion and not by expulsion, by heating and not by cooling. . . . —Yours very truly,

ALFRED R. WALLACE.

PART VI.—(Continued)

II.—Spiritualism

"The completely materialistic mind of my youth and early manhood has been slowly moulded into the socialistic, spiritualistic, and theistic mind I now exhibit—a mind which is, as my scientific friends think, so weak and credulous in its declining years, as to believe that fruit and flowers, domestic animals, glorious birds and insects, wool, cotton, sugar and rubber, metals and gems, were all foreseen and foreordained for the education and enjoyment of man. The whole cumulative argument of my 'World of Life' is that *in its every detail* it calls for the agency of a mind . . . enormously above and beyond any human mind . . . whether this Unknown Reality is a single Being and acts everywhere in the universe as direct creator, organizer, and director of every minutest motion . . . or through 'infinite grades of beings,' as I suggest, comes to much the same thing. Mine seems a more clear and intelligible supposition . . . and it is the teaching of the Bible, of Swedenborg, and of Milton."—Letter from A. R. Wallace to JAMES MARCHANT, written in 1913.

THE letters on Spiritualism which Wallace wrote cast further light on the personal attitude of mind which he maintained towards that subject. He was an unbiased scientific investigator, commencing on the "lower level" of spirit phenomena, such as raps and similar physical manifestations of "force by unseen intelligences," and passing on to a clearer understanding of the phenomena of mesmerism and telepathy; to the materialisation of, and conversation with, the spirits of those who had been known in the body, until the conviction of life after death, as the inevitable crowning conclusion to the long process of evolution, was reached in the remarkable chapter with which he concludes "The World of Life"—an impressive prose poem.

Like that of many other children, Wallace's early childhood was spent in an orthodox religious atmosphere, which, whilst awakening within him vague emotions of religious fervour, derived chiefly from the more picturesque and impassioned of

the hymns which he occasionally heard sung at a Nonconformist chapel, left no enduring impression. Moreover, at the age of 14 he was brought suddenly into close contact with Socialism as expounded by Robert Owen, which dispelled whatever glimmerings of the Christian faith there may have been latent in his mind, leaving him for many years a confirmed materialist.

This fact, together with his early-aroused sense of the social injustice and privations imposed upon the poorer classes both in town and country, which he carefully observed during his experience as a land surveyor, might easily have had an undesirable effect upon his general character had not his intense love and reverence for nature provided a stimulus to his moral and spiritual development. But the "directive Mind and Purpose" was preparing him silently and unconsciously until his "fabric of thought" was ready to receive spiritual impressions. For, according to his own theory, as "the laws of nature bring about continuous development, on the whole progressive, one of the subsidiary results of this mode of development is that no organ, no sensation, no faculty arises *before* it is needed, or in greater degree than it is needed."¹ From this point of view we may make a brief outline of the manner in which this particular "faculty" arose and was developed in him.

When at Leicester, in 1844, his curiosity was greatly excited by some lectures on mesmerism given by Mr. Spencer Hall, and he soon discovered that he himself had considerable power in this direction, which he exercised on some of his pupils.

Later, when his brother Herbert joined him in South America, he found that he also possessed this gift, and on several occasions they mesmerised some of the natives for mere amusement. But the subject was put aside, and Wallace paid no further attention to such phenomena until after his return to England in 1862.

It was not until the summer of 1865 that he witnessed any phenomena of a spiritualistic nature; of these a full account is given in "Miracles and Modern Spiritualism" (p. 132). "I came," he says, "to the inquiry utterly inbiased by hopes or fears, because I knew that my belief could not affect the reality, and with an ingrained prejudice even against such a word as 'spirit,' which I have hardly yet overcome."

¹ "The World of Life," p. 374.

From that time until 1895, when the second edition of that book appeared, he did much, together with other scientists, to establish these facts, as he believed them to be, on a rational and scientific foundation. It will also be noticed, both before and after this period, that in addition to the notable book which he published dealing exclusively with these matters, the gradual trend of his convictions, advancing steadily towards the end which he ultimately reached, had become so thoroughly woven into his "fabric of thought" that it appears under many phases in his writings, and occupies a considerable part of his correspondence, of which we have only room for some specimens.

The first definite statement of his belief in "this something" other than material in the evolution of Man appeared in his essay on "The Development of Human Races under the Law of Natural Selection" (1864). In this he suggested that, Man having reached a state of physical perfection through the progressive law of Natural Selection, thenceforth Mind became the dominating factor, endowing Man with an ever-increasing power of intelligence which, whilst the physical had remained stationary, had continued to develop according to his needs. This "in-breathing" of a divine Spirit, or the controlling force of a supreme directive Mind and Purpose, which was one of the points of divergence between his theory and that held by Darwin, is too well known to need repetition.

This disagreement has a twofold interest from the fact that Darwin, in his youth, studied theology with the full intention of taking holy orders, and for some years retained his faith in the more or less orthodox beliefs arising out of the Bible. But as time went by, an ever-extending knowledge of the mystery of the natural laws governing the development of man and nature led him to make the characteristically frank avowal that he "found it more and more difficult . . . to invent evidence which would suffice to convince"; adding, "This disbelief crept over me at a very slow rate, but was at last complete. The rate was so slow that I felt no distress."¹ With Wallace, however, his early disbelief ended in a deep conviction that "as nothing in nature actually 'dies,' but renews its life in another and higher form, so Man, the highest product of natural laws here, must

¹ "Life and Letters," i. 58.

by the power of mind and intellect continue to develop hereafter."

The varied reasons leading up to this final conviction, as related by himself in "Miracles and Modern Spiritualism" and "My Life," are, however, too numerous and detailed to be retold in a brief summary in this place.

The correspondence that follows deals entirely with investigations on this side of the Atlantic, but a good deal of evidence which to him was conclusive was obtained during his stay in America, where Spiritualism has been more widely recognised, and for a much longer period than in England.

Some of the letters addressed to Miss Buckley (afterwards Mrs. Fisher) reveal the extreme caution which he both practised himself and advocated in others when following up any experimental phase of spiritual phenomena. The same correspondence also gives a fairly clear outline of his faith in the ascending scale from the physical evidence of spirit-existence to the communication of some actual knowledge of life as it exists beyond the veil.

In spiritual matters, as in natural science, though at times his head may have appeared to be "in the clouds," his feet were planted firmly on the earth. This is seen, to note another curious instance, in his correspondence with Sir Wm. Barrett, where he maintains a delicate balance between natural science and "spirit impression" when discussing the much controverted reality of "dowsing" for water.

It was this breadth of vision, unhampered by mere intellectualism, but always kept within reasonable bounds by scientific deduction and analysis, which constituted Alfred Russel Wallace a seer of the first rank.

Wallace lived to see the theory of evolution applied to the life history of the earth and the starry firmament, to the development of nations and races, to the progress of mind, morals and religion, even to the origin of consciousness and life—a conception which has completely revolutionised man's attitude towards himself and the world and God. Evolution became intelligible in the light of that idea which came to him in his hut at Ternate and changed the face of the universe. Surely it was enough for any one man to be one of the two chief originators of such a far-reaching thought and to witness its impact upon the ancient

story of special creations which it finally laid in the dust. But Wallace was privileged beyond all the men of his generation. He lived to see many of the results of the theory of evolution tested by time and to foresee that there were definite limits to its range, that, indeed, there were two lines of development—one affecting the visible world of form and colour and the other the invisible world of life and spirit—two worlds springing from two opposite poles of being and developing *pari passu*, or, rather, the spiritual dominating the material, life originating and controlling organisation. It was, in short, his peculiar task to reveal something of the Why as well as the How of the evolutionary process, and in doing so verily to bring immortality to light.

The immediate exciting cause of this discovery of the inadequacy of evolution from the material side alone to account for the world of life may seem to many to have been trivial and unworthy of the serious attention of a great scientist. How, it might be asked, could the crude and doubtful phenomena of Spiritualism afford reasonably adequate grounds for challenging its supremacy and for setting a limit to its range? But spiritualistic phenomena were only the accidental modes in which the other side of evolution struck in upon his vision. They set him upon the other track and opened up to him the vaster kingdom of life which is without beginning, limit or end; in which perchance the sequence of life from the simple to the complex, from living germ to living God, may also be the law of growth. It is in the light of this ultimate end that we must judge the stumbling steps guided by raps and visions which led him to the ladder set up to the stars by which connection was established with the inner reality of being. That was the distinctive contribution which he made to human beliefs over and above his advocacy of pure Darwinism.

Reading almost everything he could obtain upon occult phenomena, Wallace found that there was such a mass of testimony by men of the highest character and ability in every department of human learning that he thought it would be useful to bring this together in a connected sketch of the whole subject. This he did, and sent it to a secularist magazine, in which it appeared in 1866, under the title of "The Scientific Aspect of the Supernatural." He sent a copy to Huxley.

TO T. H. HUXLEY

9 *St. Mark's Crescent, Regent's Park, N.W.* November 22, 1866.

Dear Huxley,—I have been writing a little on a *new branch* of Anthropology, and as I have taken your name in vain on the title-page I send you a copy. I fear you will be much shocked, but I can't help it; and before finally deciding that we are all mad I hope you will come and see some very curious phenomena which we can show you, *among friends only*. We meet every Friday evening, and hope you will come sometimes, as we wish for the fullest investigation, and shall be only too grateful to you or anyone else who will show us how and where we are deceived.

T. H. HUXLEY TO A. R. WALLACE

[? November, 1866.]

Dear Wallace,—I am neither shocked nor disposed to issue a Commission of Lunacy against you. It may be all true, for anything I know to the contrary, but really I cannot get up any interest in the subject. I never cared for gossip in my life, and disembodied gossip, such as these worthy ghosts supply their friends with, is not more interesting to me than any other. As for investigating the matter, I have half-a-dozen investigations of infinitely greater interest to me to which any spare time I may have will be devoted. I give it up for the same reason I abstain from chess—it's too amusing to be fair work, and too hard work to be amusing.—Yours faithfully,

T. H. HUXLEY.

TO T. H. HUXLEY

9 *St. Mark's Crescent, Regent's Park, N.W.* December 1, 1866.

Dear Huxley,—Thanks for your note. Of course, I have no wish to press on you an inquiry for which you have neither time nor inclination. As for the "gossip" you speak of, I care for it as little as you can do, but what I do feel an intense interest in is the exhibition of *force* where force has been declared *impossible*, and of *intelligence* from a source the very mention of which has been deemed an *absurdity*.

Faraday has declared (apropos of this subject) that he who can prove the existence or exertion of force, if but the lifting of a single ounce, by a power not yet recognised by science, will deserve and assuredly receive applause and gratitude. (I quote from memory the sense of his expressions in his Lecture on Education.)

I believe I can now show such a force, and I trust some of the physicists may be found to admit its importance and examine into it.—Believe me yours very sincerely,

ALFRED R. WALLACE.

TO MISS BUCKLEY

Holly House, Barking, E. December 25, 1870.

Dear Miss Buckley, — . . . You did not hear Mrs. Hardinge¹ on very favourable topics, and I hope you will hear her often again, and especially hear one of her regular discourses. I think, however, from what you heard, that, setting aside all idea of her being more than a mere spiritualist lecturer setting forth the ideas and opinions of the sect, you will admit that spiritualists, as represented by her, are neither prejudiced nor unreasonable, and that they are truly imbued with the scientific spirit of subordinating all theory to fact. You will also admit, I think, that the moral teachings of Spiritualism, as far as she touched upon them, are elevated and beautiful and calculated to do good; and if so, that is the use of Spiritualism—the getting such doctrines of future progress founded on actual phenomena which we can observe and examine now, not on phenomena which are said to have occurred thousands of years ago and of which we have confessedly but imperfect records.

I think, too, that the becoming acquainted with two such phases of Spiritualism as are exhibited by Mrs. Hardinge and Miss Houghton must show you that the whole thing is not to be judged by the common phenomena of public séances alone, and I can assure you that there are dozens of other phases of the subject as remarkable as these two. . . . —Yours very faithfully,

ALFRED R. WALLACE.

¹ Considerable reference is made to Mrs. Hardinge in "Miracles and Modern Spiritualism," pp. 117-21.

TO MISS BUCKLEY

Holly House, Barking, E. June 1, 1871.

Dear Miss Buckley,— . . . I have lately had a séance with the celebrated Mr. Home, and saw that most wonderful phenomenon an accordion playing beautiful music by itself, the bottom only being held in Mr. Home's hand. I was invited to watch it as closely as I pleased under the table in a well-lighted room. I am sure nothing touched it but Mr. Home's one hand, yet at one time I saw a shadowy yet defined hand on the keys. This is too vast a phenomenon for any sceptic to assimilate, and I can well understand the impossibility of their accepting the evidence of their own senses. Mr. Crookes, F.R.S., the chemist, was present and suspended the table with a spring balance, when it was at request made heavy or light, the indicator moving accordingly, and to prevent any mistake it was made light when the hands of all present were resting on the table and heavy when our hands were all underneath it. The difference, if I remember, was about 40 lb. I was also asked to place a candle on the floor and look under the table while it was lifted completely off the floor, Mr. Home's feet being two feet distant from any part of it. This was in a lady's house in the West End. Mr. Home courts examination if people come to him in a fair and candid spirit of inquiry. . . . —Yours very faithfully,

ALFRED R. WALLACE.

TO MISS BUCKLEY

The Dell, Grays, Essex. January 11, 1874.

My dear Miss Buckley,—I am delighted to hear of your success so far, and hope you are progressing satisfactorily. Pray keep accurate notes of all that takes place. . . . Allow me . . . to warn you not to take it for granted till you get proof upon proof that it is really your sister that is communicating with you. I hope and think it is, but still, the conditions that render communication possible are so subtle and complex that she may not be able; and some other being, reading your mind, may be acting through you and making you think it is your sister, to induce you to go on. Be therefore on the look out for characteristic traits of your sister's mind and manner which are dif-

ferent from your own. These will be tests, especially if they come when and how you are not expecting them. Even if it is your sister, she may be obliged to use the intermediation of some other being, and in that case her peculiar idiosyncrasy may be at first disguised, but it will soon make itself distinctly visible. Of course you will preserve every scrap you write, and date them, and they will, I have no doubt, explain each other as you go on.

If you can get to see the last number of the *Quarterly Journal of Science*, you will find a most important article by Mr. Crookes, giving an outline of the results of his investigations, which he is going to give in full in a volume. His facts are most marvellous and convincing, and appear to me to answer every one of the objections that have usually been made to the evidence adduced. . . . —Yours very faithfully,

ALFRED R. WALLACE.

TO MISS BUCKLEY

The Dell, Grays, Essex. February 28, 1874.

Dear Miss Buckley,—I was much pleased with your long and interesting letter of the 19th and am glad you are getting on at last. It will be splendid if you really become a good medium for some first-rate unmistakable manifestations that even Huxley will acknowledge are worth seeing, and Carpenter confess are not to be explained by unconscious cerebration. . . . —Yours very faithfully,

ALFRED R. WALLACE.

TO MISS BUCKLEY

The Dell, Grays, Essex. March 9, 1874.

Dear Miss Buckley,—I compassionate your mediumistic troubles, but I have no doubt it will all come right in the end. The fact that your sister will not talk as you want her to talk—will not say what you expect her to say, is a grand proof that it is not your unconscious cerebration that does her talking for her. Is not that clear? Whether it is she herself or someone else who is talking to you, is not so clear, but that it is not you, I think, is clear enough.

I can quite understand, too, that your sister in her new life

may be, above all things, interested in getting the telegraph in good order, to communicate, and will not think of much else till that is done. While the first Atlantic cable was being laid the messages would be chiefly reports of progress, directions and instructions, with now and then trivialities about the weather, the time, or small items of news. Only when it was in real working order was a President's Message, a Queen's Speech, sent through it.

Automatic writing and trance speaking never yet convinced anybody. They are only useful for those who are already convinced. But you *would* begin this way. You would not go to mediums and séances and see what you could get that way. So now you must persevere; but do not give up your own judgment in anything. Insist upon having things explained to you, or say you won't go on. You will then find they will be explained, only it may take a little more time. . . . —Yours very faithfully,

ALFRED R. WALLACE.

TO MISS BUCKLEY

The Dell, Grays, Essex. April 24, 1874.

Dear Miss Buckley,— . . . On coming home this evening I received the news of poor little Bertie's death—this morning at eight o'clock. I left him only yesterday forenoon, and had then considerable hopes, for we had just commenced a new treatment which a fortnight earlier I am pretty sure might have saved him. The thought suddenly struck me to go to Dr. Williams of Hayward's Heath . . . but it was too late. As he had been in this same state of exhaustion for nearly a month, it is evident that very slight influences might have been injurious or beneficial. Our orthodox medical men are profoundly ignorant of the subtle influences of the human body in health and disease, and can thus do nothing in many cases which Nature would cure if assisted by proper conditions. We who know what strange and subtle influences are around us can believe this. . . . —Yours very truly,

ALFRED R. WALLACE.

Mr. Wallace felt the death of this child so deeply that during the remainder of his life he never mentioned him except when obliged, and then with tears in his eyes.—A. B. FISHER.

TO MISS BUCKLEY

The Dell, Grays, Essex. Thursday evening, [? December, 1875].

Dear Miss Buckley,—Our séance came off last evening, and was a tolerable success. The medium is a very pretty little lively girl, the place where she sits a bare empty cupboard formed by a frame and doors to close up a recess by the side of a fireplace in a small basement breakfast-room. We examined it, and it is absolutely impossible to conceal a scrap of paper in it. Miss Cooke is locked in this cupboard, above the door of which is a square opening about 15 inches each way, the only thing she takes with her being a long piece of tape and a chair to sit on. After a few minutes Katie's whispering voice was heard, and a little while after we were asked to open the door and seal up the medium. We found her hands tied together with the tape passed three times round each wrist and tightly knotted, the hands tied close together, the tape then passing behind and well knotted to the chair-back. We sealed all the knots with a private seal of my friend's, and again locked the door. A portable gas-lamp was on a table the whole evening, shaded by a screen so as to cast a shadow on the square opening above the door of the cupboard till permission was given to illuminate it. Every object and person in the room were always distinctly visible. A face¹ then appeared at the opening, but dark and indistinct. After a time another face quite distinct with a white turban-like head-dress—this was a handsome face with a considerable general likeness to that of the medium, but paler, larger, fuller, and older—decidedly a different face, although like. The light was thrown full on this face, and on request it advanced so that the chin projected a little beyond the aperture. We were then ordered to release the medium. I opened the door, and found her bent forward with her head in her lap, and apparently in a deep sleep or trance—from which a touch and a few words awoke her. We then examined the tape and knots—all was as we left it and every seal perfect.

The same face appeared later in the evening, and also one decidedly different with coarser features.

After this, for the sake I believe of two sceptics present, the

¹ The "spirits" are supposed to produce the faces.

medium was twice tied up in a way that no human being could possibly tie herself. Her wrists were tied together so tightly and painfully that it was impossible to untie them in any moderate time, and she was also secured to the chair; on the other occasion the two arms were tied close above the elbows so tightly that the arms were swelling considerably from impeded circulation, the elbows being drawn together as close as possible behind the back, there repeatedly knotted, and again tightly knotted to the back of the chair. Miss C. was evidently in considerable pain, and she had to be lifted out bodily in her chair before we could safely cut her loose, so tightly was she bound. This evidently had a great effect on the sceptics, as I have no doubt it was intended to have, and it demonstrated pretty clearly that some strange being was inside the cupboard playing these tricks, although quite invisible and intangible to us except when she made certain portions of herself visible.

When Miss C. was complaining of being hurt by the tying we could hear the whispering voice soothing her in the kindest manner, and also heard kisses, and Miss C. afterwards declared that she could feel hands and face about her like those of a real person.

During all the face exhibitions singing had to go on to a rather painful extent.¹

A Dr. Purdon was present, an Army surgeon, who has been much in India, and seems a very intelligent man. He seemed very intimate with the family, and told us he had studied them all, and had had Miss Cooke a month at a time in his own house, studying these phenomena. He was absolutely satisfied of their genuineness, and indeed no opportunity for imposture seems to exist.

The children of the house tell wonderful tales of how they are lifted up and carried about by the spirits. They seem to enjoy it very much, and to look upon it all as just as real and natural as any other matters of their daily life.

Can such things be in this nineteenth century, and the wise ones pass away in utter ignorance of their existence?—Yours very sincerely,

ALFRED R. WALLACE.

¹ This is a strange accompaniment of most advanced spiritual phenomena.

At the Glasgow Meeting of the British Association in 1876, Prof. (now Sir) W. F. Barrett read a paper "On some Phenomena associated with Abnormal Conditions of Mind." Wallace was Chairman of the Section in which the paper was read, and a vigorous controversy arose at the close between Dr. Carpenter, who came in towards the end of the paper, and the Chairman. The paper set forth certain remarkable evidence which Prof. Barrett had obtained from a subject in the mesmeric trance, giving what appeared to be indubitable proof of some supernatural mode of transmission of ideas from his mind to that of the subject. The facts were so novel and startling that Prof. Barrett asked for a committee of experts to examine the whole question and see whether such a thing as "thought-transference," independently of the recognised channels of sense, did really exist. This was the first time evidence of this kind had been brought before a scientific society, and a protracted discussion followed. The paper also dealt with certain so-called spiritualistic phenomena, which at the time Prof. Barrett was disposed to attribute to hallucination and "thought-transference." The introduction of this topic led the discussion away from the substance of the paper, and Prof. Barrett's plea for a committee of investigation on thought-transference fell through. So strong was the feeling against the paper in official scientific circles at the time that even an abstract was refused publication in the *Report* of the British Association, and it was not until the Society for Psychical Research was founded that the paper was published, in the first volume of its *Proceedings*. It was the need of a scientific society to collect, sift and discuss and publish the evidence on behalf of such supernatural phenomena as Prof. Barrett described at the British Association that induced him to call a conference in London at the close of 1881, which led to the foundation of the Society for Psychical Research early in 1882.

Wallace, in his letter to Prof. Barrett which follows, refers to Reichenbach's experiments with certain sensitives who declared they saw luminosity from the poles of a magnet after they had been for some time in a perfectly darkened room. Acting on Wallace's suggestion, Prof. Barrett constructed a perfectly darkened room and employed a large electro-magnet, the current for which could be made or broken by an assistant outside without the knowledge of those present in the darkened room.

Under these circumstances, and taking every precaution to prevent any knowledge of when the magnet was made active by the current, Prof. Barrett found that two or three persons, out of a large number with whom he experimented, saw a luminosity streaming from the poles of the magnet directly the current was put on. An article of Prof. Barrett's on the subject, with the details of the experiment, was published in the *Philosophical Magazine*, and also in the *Proceedings* of the Society for Psychical Research (Vol. I.).

TO PROF. BARRETT

Rosehill, Dorking. December 18, 1876.

My dear Prof. Barrett,— . . . I see you are to lecture at South Kensington the end of this month (I think), and if you can spare time to run down here and stay a night or two we shall be much pleased to see you, and I shall be greatly interested to have a talk on the subject of your paper, and hear what further evidence you have obtained. I want particularly to ask you to take advantage of any opportunity that you may have to test the power of sensitives to see the "flames" from magnets and crystals, as also to *feel* the influence from them. This is surely a matter easily tested and settled. I consider it has been tested and settled by Reichenbach, but he is ignored, and a fresh proof of this one fact, by indisputable tests, is much needed; and a paper describing such tests and proofs would I imagine be admitted into the *Proceedings* of any suitable society.

You will have heard no doubt of the Treasury having taken up the prosecution of Slade. Massey the barrister, one of the most intelligent and able of the Spiritualists (whose accession to the cause is due, I am glad to say, to my article in the *Fortnightly*), proposes a memorial and deputation to the Government protesting against this prosecution by the Treasury on the ground that it implies that Slade is an habitual impostor and nothing else, and that in face of the body of evidence to the contrary, it is an uncalled-for interference with the private right of investigation into these subjects. On such general grounds as these I sincerely hope you will give your name to the memorial. . . . —Yours very faithfully,

A. R. WALLACE.

TO PROF. BARRETT

Rosehill, Dorking. December 9, 1877.

My dear Barrett,—I am always glad when a man I like and respect treats me as a friend. I am advised by other friends also not to waste more time on Dr. C. [Carpenter], and I do not think I shall answer him again, except perhaps to keep him to certain points, as in my letter in the last *Nature*. In a proof of his new edition of "Lectures" I see he challenges me to produce a person who can detect by light or sensation when an electro-magnet is made and unmade. The Association of Spiritualists are going to experiment, as Dr. C. offers to pay £30 if it succeeds. Should you have an opportunity of trying with any persons, and can find one who sees or feels the influence strongly, it might be worth while to send him to London, as nothing would tend to lower Dr. C. in public estimation on this subject more than his being forced to acknowledge that what he has for more than thirty years declared to be purely subjective is after all an objective phenomenon.

I never had anything to do with showing or sending a medium to Huxley. He must refer to his séance a few months ago with Mrs. Kane and Mrs. Jencken (along with Carpenter and Tyn-dall), when . . . nothing but raps occurred. . . . —Yours very faithfully,

ALFRED R. WALLACE.

The British Association met in Dublin in 1878, and Prof. Barrett asked Wallace to stay with him at Kingstown, or, if he preferred being nearer the meetings, with a friend in Dublin. Earlier in the year Mr. Huggins, afterwards Sir W. Huggins, O.M. and President of the Royal Society, had sent Prof. Barrett a very beautifully executed drawing of the knots tied in an endless cord during the remarkable sittings Prof. Zöllner had with the medium Slade. Sir W. Huggins invited Prof. Barrett to come and see him at his observatory at Tulse Hill, near London, and there he met Wallace and discussed the whole matter. It may not be generally known that so careful and accurate an observer as Sir W. Huggins was convinced of the genuineness of the phenomena he had witnessed with Lord Dunraven and others through the medium D. D. Home. He informed Prof. Barrett of this himself.

TO PROF. BARRETT

Waldron Edge, Duppas Hill, Croydon. June 27, 1878.

My dear Barrett,—The receipt of a British Association circular reminds me of your kind invitation to stay with you or your friend at Dublin, and as you may be wishing soon to make your arrangements I write at once to let you know that, much to my regret, I shall not be able to come to Dublin this year. Since I met you at Mr. Huggins's I have done nothing myself in Spiritual investigations, but have been exceedingly interested in the knot-tying experiment of Prof. Zöllner and the weight-varying experiments of the Spiritualists' Association. I do not see what flaw can be found in either of them. . . . —Yours very faithfully,

ALFRED R. WALLACE.

In the discussion on Prof. Barrett's paper at the Glasgow Meeting of the British Association, which took place in the *London Times* and other newspapers, instances of apparent thought-transference were given by many correspondents. Each of these cases Prof. Barrett investigated personally, and one of them led to a remarkable series of experiments which he conducted at Buxton, with the result that no doubt was left on his mind of the fact of the transference of ideas from one mind to another independent of the ordinary channels of sense. He asked Prof. and Mrs. H. Sidgwick to come to Buxton and repeat his experiments with the subjects there—daughters of a local clergyman. They did so, and though they had less success at first than Prof. Barrett had had, they were ultimately convinced of the genuineness of the phenomena. In addition, Mr. Edmund Gurney, Mr. Frederic Myers, Prof. A. Hopkinson and Prof. Balfour Stewart, all responded to Prof. Barrett's invitation to visit Buxton and test the matter for themselves, and all came to the same conclusion as he had. Subsequently Gurney and Myers associated their name with Barrett's in a paper on the subject, published in the *Nineteenth Century*.

Prof. Barrett asked Wallace to read over the first report made by Prof. and Mrs. Sidgwick, which at first seemed somewhat disheartening, and the following is his reply:

REMARKS ON EXPERIMENTS IN THOUGHT READING BY MR.
AND MRS. SIDGWICK AT BUXTON

The failure of so many of these experiments seems to me to depend on their having been conducted without any knowledge of the main peculiarity of thought reading or clairvoyance—that it is a perception of the object thought of or hidden, not by its name, or even by its sum total of distinctive qualities, but by the simple qualities separately. A clairvoyant will perceive a thing as round, then as yellow, and finally as an orange. Now Mr. Galton's experiments have shown how various are the powers of visualising objects possessed by different persons, and how distinct their modes of doing so; and if these distinct visualisations of the same thing are in any way presented to a clairvoyant, there is little wonder that some confusion should result. This would suggest that one person who possesses the faculty of clearly visualising objects would meet with more success than a number of persons some of whom visualise one portion or quality of the object, some another, while to others the name alone is present to the mind. It follows from these considerations that cards are bad for such experiments. The qualities of number, colour, form and arrangement may be severally most prominent in one mind or other, and the result is confusion to the thought reader. This is shown in the experiments by the number of pips or the suit alone being often right.

It must also be remembered that children have not the same thorough knowledge of the names of the cards that we have, nor can they so rapidly and certainly count their numbers. This introduces another source of uncertainty which should be avoided in such experiments as these.

The same thing is still more clearly shown by the way in which objects are guessed by some prominent quality or resemblance, not by any likeness of name—as poker guessed for walking-stick, fork for pipe, something iron for knife, etc. And the total failure in the case of names of towns is clearly explained by the fact that these would convey no distinct idea or concrete image that could be easily described. These last failures really give an important clue to the nature of the faculty that is being investigated, since they show that it is not *words* or *names* that are read but thoughts or images that are perceived, and the certainty of

the perception will depend upon the simple character of these images and the clearness and identity of the perception of them by the different persons present.

If these considerations are always kept in view, I feel sure that the experiments will be far more successful.

ALFRED R. WALLACE.

Sept. 6, 1881.

Wallace's remarkable gifts as a lecturer are less widely known than his lucid and admirable style as a writer. Though Sir Wm. Barrett has heard a great number of eminent scientific men lecture, he considers that few could approach him for the simplicity, clearness and vigour of his exposition, which commanded the unflagging attention of every one of his hearers. Mr. Frederic Myers, no mean judge of literary merit, once said he thought Wallace one of the most lucid English writers and lecturers of his time. Prof. Barrett was anxious to induce Wallace to lecture in Dublin, and brought the matter before the Science Committee of the Royal Dublin Society, which arranges a course of afternoon lectures by distinguished men every spring. The Committee cordially supported the suggestion that Wallace should be invited to lecture, and the invitation was accepted. During his visit to Dublin, Wallace stayed with Prof. Barrett at Kingstown, and was busily engaged in revising the proof-sheets of his book on "Land Nationalisation" (1882).

In "My Life" (Vol. II., p. 334) Wallace says that among the eminent men whose "first acquaintance and valued friendship" he owed to a common interest in Spiritualism was Frederic Myers, whom he met first at some séances in London about the year 1878.

F. W. H. MYERS TO A. R. WALLACE

Leckhampton House, Cambridge. April 12, 1890.

My dear Wallace,—I will read your pamphlet ¹ most carefully; will write and tell you how it affects me; and will in any case send it on with your letter and a letter of my own to Sir John Gorst, whom I know well, and whom I agree with you in regarding as the most acceptable member of the Government.

¹ Against vaccination.

If I am converted, it will be wholly *your* doing. I have read much on the subject—Creighton, etc., and am at present strongly pro-vaccination; at the same time, there is no one by whom I would more willingly be converted than yourself.

I am glad to take this opportunity of telling you something about my relation to one of your books. I write now from bed, having had some influenzic pneumonia, now going off. For some days my temperature was 105 and I was very restless at night, anxious to read, but in too sensitive and fastidious a state to tolerate almost any book. I found that almost the only book which I could read was your "Malay Archipelago" (of course I had read it before). In spite of my complete ignorance of natural history there was a certain charm about the book, both moral and literary, which made it deeply congenial in those trying hours. You have had few less instructed readers, but very few can have dwelt on that simple manly record with a more profound sympathy.

I want to bespeak you as a *friend at court*. When we get into the next world, I beg you to remember me and say a good word for me when you can, as you will have much influence there.

To me it seems that Hodgson's report¹ is the *best* thing which we have yet published. I trust that it impresses you equally. It has converted *Podmore* amongst other people!

I will, then, write again soon, and I am yours most truly,
F. W. H. MYERS.

TO MRS. FISHER (*née* BUCKLEY)

Parkstone, Dorset. January 4, 1896.

My dear Mrs. Fisher,—I am glad to hear that you are going on with your book. I am sure it will be a comfort to you. I have read one book of Hudson's—"A Scientific Demonstration of a Future Life," and that is so pretentious, so unscientific, and so one-sided that I do not feel inclined to read more of the same author's work. I do not think I mentioned to you (as I thought you did not read much now) a really fine and original work, called "Psychic Philosophy, a Religion of Natural Law," by Deseris (Redway). I should like to know if, after reading that, you still think Hudson's books worth reading.

¹ Psychical Research Society Report.

I have been much pleased and interested lately in reading Mark Twain's, Mrs. Oliphant's and Andrew Lang's books about Joan of Arc. The last two are far the best, Mrs. Oliphant's as a genuine sympathetic *history*, Lang's as a fine realistic story ("A Monk of Fife"). Jeanne was really perhaps the most beautiful character in authentic history, and the one that most conclusively demonstrates spirit-guidance, and both Mrs. Oliphant and A. Lang bring this out admirably. . . . —Yours very faithfully,

ALFRED R. WALLACE.

TO MRS. FISHER

Parkstone, Dorset. September 14, 1896.

My dear Mrs. Fisher,—I have much pleasure in signing your application for the Psychical Research Society, though the majority of the active members are so absurdly and illogically sceptical that you will not find much instruction in their sayings. Mr. Podmore's Report in the last issued *Proceedings* is a good illustration. . . .

We have all been in Switzerland this year. Violet, her mother, and five lady friends all went together to a rather newly-discovered place, Adelboden, a branch valley from that going up to the Gemmi Pass by Kandersteg. I went first for a week to Davos, to give a lecture to Dr. Lunn's party, and enjoyed myself much, chiefly owing to the company of Rev. Hugh Price Hughes, one of the most witty, earnest, advanced, and estimable men I have ever met. Dr. Lunn himself is very jolly, and we had also Mr. Le Gallienne, the poet and critic, and between them we had a very brilliant table-talk. Mr. Haweis was also there, and one afternoon he and I talked for two hours about Spiritualism. He is a thorough spiritualist, and preaches it. . . . —Yours very sincerely,

ALFRED R. WALLACE.

TO MRS. FISHER

Parkstone, Dorset. April 9, 1897.

My dear Mrs. Fisher,—I have tried several Reincarnation and Theosophical books, but *cannot* read them or take any interest in them. They are so purely imaginative, and do not seem to me rational. Many people are captivated by it—I

think most people who like a grand, strange, complex theory of man and nature, given with authority—people who if religious would be Roman Catholics. Crookes gave a suggestive and interesting, but in some ways rather misleading address as President of the Psychical Research Society. I liked Oliver Lodge's address to the Spiritualists' Association better. . . . —Yours very sincerely,

ALFRED R. WALLACE.

In 1891, at the urgent request of Prof. H. Sidgwick, President of the Society for Psychical Research, Prof. Barrett undertook, with considerable reluctance, to make a thorough examination of the subject of "dowsing" for water and minerals by means of the so-called "divining rod." At the time he fully believed that a critical inquiry of this kind would speedily show all the alleged successes of the dowser to be due either to fraud or a sharp eye for the ground. As the inquiry went on, to his surprise he found that neither chicanery, nor clever guessing, nor local knowledge, nor chance coincidence could explain away the accumulated evidence, but that something new to science was really at the root of the matter. This result was so startling that Prof. Barrett had to pursue the investigation for six years before venturing to publish his first report, which appeared in the *Proceedings* of the Society for Psychical Research, Part xxxii., 1897. This was followed by a second report published some years later, in which he gave a fresh body of evidence on the criticisms of some eminent geologists to whom he had submitted the evidence. The reports were reviewed in *Nature* with considerable severity, and some erroneous statements were made, to which Prof. Barrett replied. The editor, Sir Norman Lockyer, at first declined to publish Prof. Barrett's reply, and to this Wallace refers in the following letter.

TO PROF. BARRETT

Parkstone, Dorset. October 30, 1899.

My dear Barrett,— . . . Apropos of *Nature*, they never gave a word of notice to my book ¹—probably they would say out of

¹ "The Wonderful Century."

kindness to myself as one of their oldest contributors, since they would have had to scarify me, especially as regards the huge Vaccination chapter, which is nevertheless about the most demonstrative bit of work I have done. I begged Myers—as a personal favour—to read it. He told me he firmly believed in vaccination, but would do so, and afterwards wrote me that he could see no answer to it, and if there was none he was converted. There certainly has been not a tittle of answer except abuse.

I am glad you brought Lockyer up sharp in his attempt to refuse you the right to reply. I am glad you now have some personal observations to adduce. I hope persons or corporations who are going to employ a dowser will now advise you so that you may be present. . . . —Yours very faithfully,

ALFRED R. WALLACE.

TO PROF. BARRETT

Parkstone, Dorset. December 24, 1900.

My dear Barrett,— . . . I have read your very interesting paper on the divining rod, and the additional evidence you now send. Of course, I think it absolutely conclusive, but there are many points on which I differ from your conclusions and remarks, which I think are often unfair to the dowzers.

I will just refer to one or two. At p. 176 (note) you call the idea of there being a “spring-head” at a particular point “absurd.” But instead of being absurd it is a *fact*, proved not only by numerous cases you have given of strong springs being found quite near to weak springs a few yards off, but by all the phenomena of mineral and hot springs. Near together, as at Bath, hot springs and cold springs rise to the surface, and springs of different quality at Harrogate, yet each keeps its distinct character, showing that each rises from a great depth without any lateral diffusion or intermixture. This is a common phenomenon all over the world, the dowzers’ facts support it, geologists know all about it, yet I presume they have told you that when a dowser states this fact it ceases to be a fact and becomes an absurdity!

The only other point I have time to notice is your Sect. II. (p. 285). You head this, “Evidence that the Motion of the Rod is

due to Unconscious Muscular Action." Naturally I read this with the greatest interest, but found to my astonishment that you adduce no evidence at all, but only opinions of various people, and positive assertions that such is the case! Now as I *know* that motions of various objects occur without any muscular action, or even any contact whatever, while Crookes has proved this by careful experiments which have never been refuted, what *improbability* is there that this should be such a case, and what is the value of these positive assertions which you quote as "evidence"? And at p. 286 you quote the person who says the more he tried to prevent the stick's turning the more it turned, as *evidence* in favour of muscular action, without a word of explanation. Another man (p. 287) says he "could not restrain it." None of the "trained anatomists" you quote give a particle of *proof*, only positive opinion, that it must be muscular action—simply because they do not believe any other action possible. Their evidence is just as valueless as that of the people who say that all thought-transference is collusion or imposture!

I do not say that it is not "muscular action," though I believe it is not always so, but I do say that you have as yet given not a particle of proof that it is so, while scattered through your paper is plenty of evidence which points to its being something quite different. Such are the cases when people hold the rod for the first time and have never seen a dowser work, yet the rod turns, over water, to their great astonishment, etc., etc.

Your conclusion that it is "clairvoyance" is a good provisional conclusion, but till we know what clairvoyance really is it explains nothing, and is merely another way of stating the *fact*.

I believe all true clairvoyance to be spirit impression, and that all true dowsing is the same—that is, when in either case it cannot be thought-transference, but even this I believe to be also, for the most part, if not wholly, spirit impression.—Believe me yours very truly,

ALFRED R. WALLACE.

TO PROF. BARRETT

Parkstone, Dorset. February 17, 1901.

My dear Barrett,—I am rather sorry you wrote to any one of the Society for Psychical Research people about my being

asked to be President, because I should certainly feel compelled to decline it. I never go, willingly, to London now, and should never attend meetings, so pray say no more about it. Besides, I am so widely known as a "crank" and a "faddist" that my being President would injure the Society, as much as Lord Rayleigh would benefit it, so pray do not put any obstacle in *his* way, though of course there is no necessity to beg him as a favour to be the successor of Sidgwick, Crookes and Myers. . . .

TO REV. J. B. HENDERSON

Parkstone, Dorset. August 10, 1893.

Dear Sir,—Although I look upon Christianity as originating in an unusual spiritual influx, I am not disposed to consider [it] as *essentially* different from those which originated other great religious and philanthropic movements. It is probable that in *your* sense of the word I am not a Christian.—Believe me yours very truly,

ALFRED R. WALLACE.

TO J. W. MARSHALL

Parkstone, Dorset. March 6, 1894.

My dear Marshall,—We were very much grieved to hear of your sad loss in a letter from Violet. Pray accept our sincere sympathy for Mrs. Marshall and yourself.

Death makes us feel, in a way nothing else can do, the mystery of the universe. Last autumn I lost my sister, and she was the only relative I have been with at the last. For the moment it seems unnatural and incredible that the living self with its special idiosyncrasies you have known so long can have left the body, still more unnatural that it should (as so many now believe) have utterly ceased to exist and become nothingness!

With all my belief in, and knowledge of, Spiritualism, I have, however, occasional qualms of doubt, the remnants of my original deeply ingrained scepticism; but my reason goes to support the psychical and spiritualistic phenomena in telling me that there *must* be a hereafter for us all. . . . —Believe me yours very sincerely,

ALFRED R. WALLACE.

TO DR. EDWIN SMITH

Parkstone, Dorset. October 19, 1899.

Dear Sir,—I know nothing of London mediums now. Nine-tenths of the alleged frauds in mediums arise from the ignorance of the sitters. The only way to gain any real knowledge of spiritualistic phenomena is to follow the course pursued in all science—study the elements before going to the higher branches. To expect proof of materialisation before being satisfied of the reality of such simpler phenomena as raps, movements of various objects, etc., etc., is as if a person began chemistry by trying to analyse the more complex vegetable products before he knew the composition of water and the simplest salts.

If you want to *know* anything about Spiritualism you should experiment yourself with a select party of earnest inquirers—personal friends. When you have thus satisfied yourself of the existence of a considerable range of the physical phenomena and of many of the obscurities and difficulties of the inquiry, you may use the services of public mediums, without the certainty of imputing every little apparent suspicious circumstance to trickery, since you will have seen similar suspicious facts in your private circle where you *knew* there was no trickery. You will find rules for forming private circles in some issues of *Light*. You can get them from the office of *Light*.—Yours very truly,

ALFRED R. WALLACE.

PROF. BARRETT TO A. R. WALLACE

6 De Vesce Terrace, Kingstown, Co. Dublin. November 3, 1905.

My dear Wallace,— . . . Just now I am engaged in a correspondence with the Secretaries of the Society for Psychical Research on the question of the Presidency for next year. I maintain that as a matter of duty to the Society you should be asked to accept the Presidency, though of course it would be impossible for you to be much more than an Honorary President, as we could not expect you often to come to London. I am anxious that in our records for future reference your Presidency should appear. . . . Podmore, who is proposed as President, represents the attitude of resolute incredulity, and I consider this line of action has been to some extent injurious to the S.P.R.

Crookes supported my proposal, and so did Lodge, and so would Myers if he had lived. All this is of course between ourselves. . . .

I have a vast amount of material unpublished on "dowsing" and am convinced the explanation is subconscious clairvoyance. . . . —Yours very sincerely, W. F. BARRETT.

TO MRS. FISHER

Broadstone, Wimborne. April 20, 1906.

My dear Mrs. Fisher,—If you mean "honest" by "thoroughly reliable," there are plenty of such mediums, but if you mean those who give equally good results always, and to all persons, I should say there are none. . . .

I am reading Herbert Spencer's "Autobiography" (just finished Vol. I.). I find it very interesting, though tedious in parts. I am glad I did not read it before I wrote mine. He certainly brings out his own character most strikingly, and a wonderful character it was. How extraordinarily little he owed either to teaching or to reading! I think he is best described as a "reasoning genius."—Yours very truly,

ALFRED R. WALLACE.

LORD AVEBURY TO A. R. WALLACE

48 Grosvenor Street, W. May 1, 1910.

My dear Wallace,—I have been reading your biography with great interest. It must be a source of very pleasant memories to you to look back and feel how much you have accomplished.

It surprises me, however, how much we differ, and it is another illustration of the problems [?] of our (or rather I should say of my) intellect.

In some cases, indeed, the difference is as to facts.

You would, I am sure, for instance, find that you have been misinformed as to "thousands of dogs" being vivisected annually (p. 392). . . . As to Spiritualism, my difficulty is that nothing comes of it. What has been gained by your séances, compared to your studies?

I see you have a kindly reference to our parties at High Elms in old days, on which I often look back with much pleasure, but much regret also.

If you would give us the pleasure of another visit, *do* propose yourself, and you will have a very hearty welcome from yours very sincerely,

AVEBURY.

A lecture delivered by Prof. Barrett before the Quest Society in London, entitled "Creative Thought," was published by request, and as it discussed the subject of evolution and the impossibility of explaining the phenomena of life without a supreme Directing and Formative Force behind all the manifestations of life, he was anxious to have Wallace's criticisms. At that time he had not read Wallace's recently published work on a similar subject, and he was greatly surprised to find how closely his views agreed with those of the great naturalist.

TO PROF. BARRETT

Old Orchard, Broadstone, Wimborne. February 15, 1911.

My dear Barrett,—Thanks for your proofs, which I return. It is really curious how closely your views coincide with mine, and how admirably and clearly you have expressed them. If it were not for your adopting throughout, as an actual fact, the (to me) erroneous theory of the "subconscious self," I should agree with every word of it. I have put "?" where this is prominently put forward, merely to let you know how I totally dissent from it. To me it is pure assumption, and, besides, proves nothing. Thanks for the flattering "Postscript," which I return with a slight suggested alteration.

Reviews have been generally very fair, complimentary and flattering. But to me it is very curious that even the religious reviewers seem horrified and pained at the idea that the Infinite Being does not actually do every detail himself, apparently leaving his angels, and archangels, his seraphs and his messengers, which seem to exist in myriads, according to the Bible, to have no function whatever!—Yours very truly,

ALFRED R. WALLACE.

PROF. BARRETT TO A. R. WALLACE

6 De Vesci Terrace, Kingstown, Co. Dublin. February 18, 1911.

My dear Wallace,— . . . Thank you very much for your kind letter and comments. I have modified somewhat the phrase-

ology as regards the "subliminal self." I think we really agree but use different terms. There *is* a hidden directive power, which works in conjunction with, and is temporarily part of, our own conscious self; but it is below the threshold of consciousness, or is a subliminal part of our self.

I should like to have come over to Broadstone expressly to ask your views on the parts you queried. For I have an immense faith in the soundness of your judgment, and in the accuracy of your views *in the long run*.

I should like also immensely to see you again and in your lovely home. . . . —Yours ever sincerely,

W. F. BARRETT.

TO PROF. BARRETT

Old Orchard, Broadstone, Wimborne. February 20, 1911.

My dear Barrett,—I wrote you yesterday on quite another matter, but having yours this morning in reply to my criticisms of your Address, I send a few lines of explanation. Most of my queries to your statements apply solely to your expressing them so positively, as if they were absolute certainties which no psychical researcher doubted. My main objection to the term "subliminal self" and its various synonyms is, that it is so dreadfully vague, and is an excuse for the assumption that a whole series of the most mysterious of psychical phenomena are held to be actually explained by it. Thus it is applied to explain all cases of apparent "possession," when the alleged "secondary self" has a totally different character, and uses the dialect of another social grade, from the normal self, sometimes even possesses knowledge that the real self could not have acquired, speaks a language that the normal self never learnt. All this is, to me, the most gross travesty of science, and I therefore object totally to the use of the term which is so vaguely and absurdly used, and of which no clear and rational explanation has ever been given.

You are now one of my oldest friends, and one with whom I most sympathise; and I only regret that we have seen so little of each other.—Yours very faithfully,

ALFRED R. WALLACE.

TO MR. E. SMEDLEY

Old Orchard, Broadstone, Dorset. October 2, 1911.

Dear Mr. Smedley,—I am quite astonished at your wasting your money on an advertising astrologer. In the horoscope sent you there is not a single definite fact that would apply to you any more than to thousands of other men. All is vague, what "might be," etc., etc. It is just calculated to lead you on to send more money, and get in reply more words and nothing else. . . . —Yours very truly,

ALFRED R. WALLACE.

PART VII

Characteristics

"There is a point of view so lofty or so peculiar that from it we are able to discern in men and women something more than and apart from creed and profession and formulated principle; which indeed directs and colours this creed and principle as decisively as it is in its turn acted on by them, and this is their character or humanity."—LORD MORLEY.

"As sets the sun in fine autumnal calm
So dost thou leave us. Thou not least but last
Link with that rare and gallant little band
Of seekers after truth, whose days, though past,
Shed lustre on the hist'ry of their land.
And thine, O Wallace, thine the added charm
Of modesty, thy mem'ry to embalm."—*Anonymous*.

(Received with a bunch of lilies-of-the-valley, a few days after Dr. Wallace's death.)

ADDISON somewhere says that modesty sets off every talent which a man can be possessed of. This was manifestly true of Alfred Russel Wallace. When, for instance, honours were bestowed upon him, he accepted or rejected them with the same good-humour and unspoilable modesty. To Prof. E. B. Poulton, whose invitation for the forthcoming Encæmia had been conveyed in Prof. Bartholomew Price's letter, he wrote:

Godalming. May 28, 1889.

My dear Mr. Poulton,—I have just received from Prof. B. Price the totally unexpected offer of the honorary degree of D.C.L. at the coming Commemoration, and you will probably be surprised and *disgusted* to hear that I have declined it. I have to thank you for your kind offer of hospitality during the ceremony, but the fact is, I have at all times a profound distaste

of all public ceremonials, and at this particular time that distaste is stronger than ever. I have never recovered from the severe illness I had a year and a half ago, and it is in hopes of restoring my health that I have let my cottage here and have taken another at Parkstone, Dorset, into which I have arranged to move on Midsummer Day. To add to my difficulties, I have work at examination papers for the next two or three weeks, and also a meeting (annual) of our Land Nationalisation Society, so that the work of packing my books and other things and looking after the plants which I have to move from my garden will have to be done in a very short time. Under these circumstances it would be almost impossible for me to rush away to Oxford except under absolute compulsion, and to do so would be to render a ceremony which at any time would be a trial, a positive punishment.

Really the greatest kindness my friends can do me is to leave me in peaceful obscurity, for I have lived so secluded a life that I am more and more disinclined to crowds of any kind. I had to submit to it in America, but then I felt exceptionally well, whereas now I am altogether weak and seedy and not at all up to fatigue or excitement.—Yours very faithfully,

ALFRED R. WALLACE.

Prof. Poulton pressed him to reconsider his decision, and he reluctantly gave way.

Godalming. June 2, 1889.

My dear Mr. Poulton,—I am exceedingly obliged by your kind letters, and I will say at once that if the Council of the University should again ask me to accept the degree, to be conferred in the autumn, as you propose, I could not possibly refuse it. At the same time I hope you will not in any way urge it upon them, as I really feel myself too much of an amateur in Natural History and altogether too ignorant (I left school—a bad one—finally, at fourteen) to receive honours from a great University. But I will say no more about that.—Yours very faithfully,

A. R. WALLACE.

In due course he received the degree. "On that occasion," says Professor Poulton, "Wallace stayed with us, and I was

anxious to show him something of Oxford; but, with all that there is to be seen, one subject alone absorbed the whole of his interest—he was intensely anxious to find the rooms where Grant Allen had lived. He had received from Grant Allen's father a manuscript poem giving a picture of the ancient city dimly seen by midnight from an undergraduate's rooms. With the help of Grant Allen's college friends we were able to visit every house in which he had lived, but were forced to conclude that the poem was written in the rooms of a friend or from an imaginary point of view."

His friend Sir W. T. Thiselton-Dyer, with others, was promoting his election to the Royal Society, and wrote to him:

SIR W. T. THISELTON-DYER TO A. R. WALLACE

Kew. October 23, 1892.

Dear Mr. Wallace,— . . . When you were at Kew this summer I took the liberty of saying that it would give great pleasure to the Fellows of the Royal Society if you would be willing to join their body. I understood you to say that it would be agreeable to you. I now propose to comply with the necessary formalities. But before doing so it will be proper to ask for your formal consent. You will then, as a matter of course, be included in the next annual election.

Will you forgive me if I am committing any indiscretion in saying that I have good authority for adding (though I suppose it can hardly be stated officially at this stage) that no demand will ever be made upon you for a subscription?—Believe me yours sincerely,

W. T. THISELTON-DYER.

SIR W. T. THISELTON-DYER TO A. R. WALLACE

Kew. January 12, 1893.

Dear Mr. Wallace,— . . . I was very vexed to hear that I had misunderstood your wishes about the Royal Society. Of course, the matter must often have presented itself to your mind, and I confess that it argued a little presumption on the part of a person like myself, so far inferior to you in age and standing, to think that you would yield to my solicitation.

I was obliged for my health to go to Eastbourne, and there I had the pleasure of seeing Mr. Huxley, who, you will be glad

to hear, is wonderfully well, and an ardent gardener! His present ambition is to grow every possible saxifrage.

I told him that I had had the audacity to approach you on the subject of the Royal Society. He heartily approved, and expressed the strongest opinion that unless you had some insuperable objection you ought to yield. All of us who belong to the R.S. have but one wish, which is that it should stand before the public as containing all that is best and worthiest in British Science. As long as men like you stand aloof, that cannot be said. Lately we have been exposed to some very ill-natured attacks: we have been told that we are professional, and not discoverers. Well, this is all the more reason for your not holding aloof from us. I wish you would think it over again. Huxley went the length of saying that to him it seemed a plain duty. But this is language I do not like to use.

As to attending the meetings or taking part in the work of the Society, that is immaterial. Darwin never did either, though he did once come to one of the evening receptions, and enjoyed it immensely.

In writing as I do I am not merely expressing my own opinions, but those of many others of my own standing who are keenly interested in the matter.

It is not a great matter to ask. I have the certificate ready. You have but to say the word. You will be put to no trouble or pecuniary responsibility. That my father-in-law arranged, long ago.

To dissociate yourself from the R.S. really amounts nowadays to doing it an injury. And I am sure you do not wish that.

With all good wishes, believe me yours sincerely,

W. T. THISELTON-DYER.

TO SIR W. T. THISELTON-DYER

Parkstone, Dorset. January 17, 1893.

Dear Mr. Thiselton-Dyer,—I have been rather unwell myself the last few days or should have answered your very kind letter sooner. I feel really overpowered. I cannot understand why you or anyone should care about my being an F.R.S., because I have really done so little of what is usually considered scientific work to deserve it. I have for many years felt almost ashamed

of the amount of reputation and honour that has been awarded me. I can understand the general public thinking too highly of me, because I know that I have the power of clear exposition, and, I think, also, of logical reasoning. But all the work I have done is more or less amateurish and founded almost wholly on other men's observations; and I always feel myself dreadfully inferior to men like Sir J. Hooker, Huxley, Flower, and scores of younger men who have extensive knowledge of whole departments of biology of which I am totally ignorant. I do not wish, however, to be thought ungrateful for the many honours that have been given me by the Royal and other Societies, and will therefore place myself entirely in your hands as regards my election to the F.R.S.

I am much pleased to hear that Huxley has taken to gardening. I have no doubt he will do some good work with his saxifragas. For myself the personal attention to my plants occupies all my spare time, and I derive constant enjoyment from the mere contemplation of the infinite variety of forms of leaf and flower, and modes of growth, and strange peculiarities of structure which are the source of fresh puzzles and fresh delights year by year.

With best wishes and many thanks for the trouble you are taking on my behalf, believe me yours very faithfully,

ALFRED R. WALLACE.

In 1902 the *Standard* announced that the degree of D.C.L. was to be conferred upon him by the University of Wales. He wrote to Miss Dora Best, who had sent him the information:

"I have not seen the *Standard*. But I suppose it is about the offer of a degree by the University of Wales. You will not be surprised to hear that I have declined it 'with thanks.' The bother, the ceremony, the having perhaps to get a blue or yellow or scarlet gown! and at all events new black clothes and a new topper! such as I have not worn this twenty years. Luckily I had a good excuse in having committed the same offence before. Some ten years back I declined the offer of a degree from Cambridge, so that settled it.

"P.S.—Having already degrees two—LL.D. (Dublin) and D.C.L. (Oxford)—I might have quoted Shakespeare: 'To gild refined gold, to paint the lily,' etc. But I didn't!—A. R. W."

In 1908 he received the Order of Merit, the highest honour conferred upon him. To his friend Mrs. Fisher he wrote:

Dear Mrs. Fisher,—Is it not awful—two more now! I should think very few men have had three such honours within six months! I have never felt myself worthy of the Copley medal—and as to the Order of Merit—to be given to a red-hot Radical, Land Nationaliser, Socialist, Anti-Militarist, etc., etc., etc., is quite astounding and unintelligible! . . .

There is another thing you have not heard yet, but it will be announced soon. Sir W. Crookes, as Secretary of the Royal Institution, wrote to me two weeks back asking me very strongly to give them a lecture at their opening meeting (third week in January) appropriate to the Jubilee of the “Origin of Species.” I was very unwell at the time—could eat nothing, etc.—and was going to decline positively, having nothing more to say! But while lying down, vaguely thinking about it, an idea flashed upon me of a new treatment of the whole subject of Darwinism, just suitable for a lecture to a R.I. audience. I felt at once there was something that ought to be said, and that I should like to say—so I actually wrote and accepted, provisionally. My voice has so broken that unless I can improve it I fear not being heard, but Crookes promised to read it either wholly, or leaving to me the opening and concluding paragraphs. I was very weak—almost a skeleton—but I am now getting much better. But finishing up the “Spruce” book, and now all these honours and congratulations and letters, etc., are giving me much work, yet I am getting strong again, and really hope to do this “lecture” as my last stroke for Darwinism against the Mutationists and Mendelians, but much more effective, I hope, than my article in the August *Contemporary Review*, though that was pretty strong.—Yours very sincerely,

ALFRED R. WALLACE.

How more than true “Sunlight’s”¹ words have come, “You will come out of the hole! You will be more in the world. You will have satisfaction, retrospection, and work”! Literally fulfilled!—A. R. W.

¹ A medium.

And to Mr. F. Birch:

December 30, 1908.

Dear Fred,— . . . I received a letter from Lord Knollys—the King's Private Secretary—informing me that His Majesty proposed to offer me the Order of Merit, among the Birthday honours! This is an "Order" established by the present King about eight years ago, solely for "merit"—whether civil or military—it is a pity it was not civil only, as the military have so many distinctions already. So I had to compose a very polite letter of acceptance and thanks, and then later I had to beg to be excused (on the ground of age and delicate health) from attending the investiture at Buckingham Palace (on December 14th), when Court dress—a kind of very costly livery—is obligatory! and I was kept for weeks waiting. But at last one of the King's Equerries, Col. Legge (an Earl's son), came down here about two weeks ago bringing the Order, which is a very handsome cross in red and blue enamel and gold—rich colours—with a crown above, and a rich ribbed-silk blue and crimson riband to hang it round the neck! Col. Legge was very pleasant, stayed half an hour, had some tea, and showed us how to wear it. So I shall be in duty bound to wear it on the only public occasion I shall be seen again (in all probability), when I give (or attempt to give) my lecture.¹ Then, I had a letter from Windsor telling me that chalk portraits of all the members of the Order were to be taken for the collections in the Library, and a Mr. Strang came and stayed the night, and in four hours completed a very good life-size head, in coloured chalk, and so far, so good!—Yours very sincerely,

ALFRED R. WALLACE.

Wallace regarded "Sunlight's" prophecy about "retrospection" as being fulfilled in 1904, when he received the invitation of Messrs. Chapman and Hall to begin collecting material for his autobiography which was subsequently published in two large volumes, under the title of "My Life."

Referring to this work he wrote to Mrs. Fisher:

¹ The lecture at the Royal Institution, when he wore the Order.

Broadstone, Dorset. April 17, 1904.

Dear Mrs. Fisher,—Thanks for your remarks on what an autobiography ought to be. But I am afraid I shall fall dreadfully short. I seem to remember nothing but ordinary facts and incidents of no interest to anyone but my own family. I do not feel myself that anything has much influenced my character or abilities, such as they are. Lots of things have given me opportunities, and those I can state. Also other things have directed me into certain lines, but I can't dilate on these; and really, with the exception of Darwin and Sir Charles Lyell, I have come into close relations with hardly any eminent men. All my doings and surroundings have been commonplace!

I am now just reading a charming and ideal bit of autobiography—Robert Dale Owen's "Threading My Way." If you have not read it, do get it (published by Trübner and Co. in 1874). It is delightful. So simple and natural throughout. But his father was one of the most wonderful men of the nineteenth century—Robert Owen of New Lanark, and this book gives the true history of his great success. Then R. D. Owen met Clarkson and heard from his own lips how he worked to abolish the slave trade.

Then he had part of his education at Hofwyl under Fellenberg, an experiment in education and self-government wonderfully original and successful. He afterwards worked at "New Harmony" with his father, and met during his life almost all the most remarkable people in England and America.

This book only contains the first twenty-seven years of his life and I am afraid he never completed it. Such a book makes me despair!—Yours very sincerely,

ALFRED R. WALLACE.

When "My Life" was published, he wrote to the same old and valued friend:

Broadstone, Wimborne. November 7, 1905.

My dear Mrs. Fisher,—The reviewers are generally very fair about the fads except a few. The *Review* invents a new word for me—I am an "anti-body"; but the *Outlook* is the richest: I am the one man who believes in spiritualism, phrenology, anti-vaccination, and the centrality of the earth in the universe,

whose life is worth writing. Then it points out a few things I am capable of believing, but which everybody else knows to be fallacies, and compares me to Sir I. Newton writing on the prophets! Yet of course he praises my biology up to the skies—there I am wise—everywhere else I am a kind of weak, babyish idiot! It is really delightful!

Only one is absolutely savage about it all—the *Liverpool Daily Post and Mercury*. The reviewer devotes over three columns almost wholly to the fads—as to all of which he evidently knows absolutely nothing, but he is cocksure that I am always wrong! . . . —Yours very sincerely,

ALFRED R. WALLACE.

He always thought that he was deficient in the gift of humour: "I am," he wrote to Mr. J. W. Marshall (May 6, 1905), "still grinding away at my autobiography. Have got to my American lecture tour, and hope to finish by about Sept. but have such lots of interruptions. I am just reading Huxley's Life. Some of his letters are inimitable, but the whole is rather monotonous. I find there is a good deal of variety in my life if I had but the gift of humour! Alas! I could not make a joke to save my life. But I find it very interesting." "Unless somebody," he wrote to Miss Evans, "can make me laugh just before the critical moment I always have a horrid expression in photographs." Yet another observant friend remarked that "he had a keen sense of humour. It was always his boyish joyous exuberance which touched me. He never grew old. When I had sat with him an hour he was a young man, he became transfigured to me." . . . "The last time I saw Dr. Wallace," writes Prof. T. D. A. Cockerell of Colorado, "was immediately after the Darwin Celebration at Cambridge in 1909. I was the first to give him the details concerning it, and vividly remember how interested he was, and how heartily he laughed over some of the funny incidents, which may not as yet be told in print. One of his most prominent characteristics was his keen sense of humour, and his enjoyment of a good story." In the summer of 1885 he spent a holiday with Prof. Meldola at Lyme Regis. "After our ramble," said the Professor, "we used to spend the evenings indoors, I reading aloud the 'Ingoldsby Legends,' which Wallace richly enjoyed. His humour was a delightful characteristic.

'The inimitable puns of T. Hood were,' he said, 'the delight of my youth, as is the more recondite and fantastic humour of Mark Twain and Lewis Carroll in my old age.'

Wallace loved to give time and trouble in aiding young men to start in life, especially if they were endeavouring to become naturalists. He sent them letters of advice, helped them in the choice of the right country to visit, and gave them minute practical instructions how to live healthily and to maintain themselves. He put their needs before other more fortunate scientific workers and besought assistance for them.

"The central secret of his personal magnetism lay in his wide and unselfish sympathy," writes Prof. Poulton.¹ "It might be thought by those who did not know Wallace that the noble generosity which will always stand as an example before the world was something special—called forth by the illustrious man with whom he was brought in contact. This would be a great mistake. Wallace's attitude was characteristic, and characteristic to the end of his life.

"A keen young naturalist in the North of England, taking part in an excursion to the New Forest, called on Wallace and confided to him the dream of his life—a first-hand knowledge of tropical nature. When I visited 'Old Orchard' in the summer of 1903, I found that Wallace was intently interested in two things: his garden, and the means by which his young friend's dream might best be realised. The subject was referred to in seventeen letters to me; it formed the sole topic of some of them. It was a grand and inspiring thing to see this great man identifying himself heart and soul with the interests of one—till then a stranger—in whom he recognised the passionate longings of his own youth. By the force of sympathy he re-lived in the life of another the splendid years of early manhood."

The late Prof. Knight recalled meeting him at the British Association in Dundee, during the year 1867, when Wallace was his guest for the usual time of the gathering. He wrote:

"I, and everyone else who then met him at my house, were struck, as no one could fail to be, by his rare urbanity, his social charm, his modesty, his unobtrusive strength, his courtesy in

¹ In *Nature*, November 20, 1913, p. 348.

explaining matters with which he was himself familiar but those he conversed with were not; and his abounding interest, not only in almost every branch of Science, but in human knowledge in all its phases, especially new ones. He was a many-sided scientific man, and had a vivid sense of humour. He greatly enjoyed anecdote, as illustrative of character. During those days he talked much on the fundamental relations between Science and Philosophy, as well as on the connection of Poetry with both of them. When he left Dundee he went to Kenmore, that he might ascend Ben Lawers in search of some rare ferns.

"In 1872 I saw him, after meeting Thomas Carlyle and Dean Stanley at Linlathen, when Darwin's theory was much discussed, and when our genial host—Mr. Erskine—talked so dispassionately but decidedly against evolution as explanatory of the rise of what was new. A little later in the same year Matthew Arnold discussed the same subject with some friends at the Athenæum Club, defending the chief aim of Darwin's theory, and enlarging from a different point of view what Wallace had done in the same direction. I remember well that he characterised the two men as fellow-workers, not as followers, or in any sense as copyists. Wallace's versatility not only continued, but grew in many ways with the advance of years. It was seen in his appreciation of the value of historical study. Quite late in life he wrote: 'The nineteenth century is quite as wonderful in the domain of History as in that of Science.' Comparatively few know, or remember, that he and his young brother Herbert—on whom he left an interesting chapter *in memoriam*—both wrote verses, some of which were of real value.

"It may be safely said that few scientific men have sympathetically entered into bordering territories and therein excelled. The whole field of psychical research was familiar to him, and he might have been a leader in it.

"My last meeting with him was at his final home, the 'Old Orchard,' Broadstone, in 1909. I was staying at Boscombe in Hants, and he asked me to 'come and see his garden, while we talked of past days.' He had then the freshness of boyhood, blent with the mellow wisdom of age."—W. A. K.

The eminent naturalist and traveller, Dr. Henry O. Forbes, who later explored the greater part of the lands visited by

Wallace, contributes the following appreciation of the latter's scientific work:

"As a traveller, explorer and working naturalist, Wallace will always stand in the first rank, compared even with the most modern explorers. It ought not to be forgotten, however, how great were the difficulties, the dangers and the cost of travel fifty years ago, compared with the facilities now enjoyed by his successors, who can command steam and motor transport to well-nigh any spot on the coasts of the globe, and who have to their hand concentrated and preserved foods, a surer knowledge of the causes of tropical diseases, and outfits of non-perishable medicines sufficient for many years within the space of a few cubic inches. Commissariat and health are the keys to all exploration in uncivilised regions. Wallace accomplished his work on the shortest of commons and lay weeks at a time sick through inability to replenish his medical stores.

"He was no mere 'trudger' over new lands. Where those before him, and even many after him, have been able to see only sterile objects, his discerning eyes perceived everywhere a meaning in the varying modes of organic life, and in response to his sympathetic mind Nature revealed more of her multitudinous secrets than to most others. Wallace's Amazonian travels were far from unfruitful, in spite of the irreparable loss he sustained in the burning of his notes and the bulk of his collections in the vessel by which he was returning home; but it was in the Malay Archipelago that his most celebrated years of investigation were passed, which marked him as one of the greatest naturalists of our time. As a methodical natural history collector—which is 'the best sport in the world' according to Darwin—he has never been surpassed; and few naturalists, if any, have ever brought together more enormous collections than he. The mere statement, taken from his 'Malay Archipelago,' of the number of his captures in the Archipelago in six years of actual collecting, exceeding 125,000 specimens—a number greater than the entire contents of many large museums—still causes amazement. The value of a collection, however, depends on the full and accurate information attached to each specimen, and from this point of view only a few collections, including Darwin's and Bates's, have possessed the great scientific value of his.

“Wallace’s Eastern explorations included nearly all the large and the majority of the smaller islands of the Archipelago. Many of them he was the first naturalist to visit, or to reside on. Ceram, Batjan, Buru, Lombock, Timor, Aru, Ke and New Guinea had never been previously scientifically investigated. When in 1858 ‘the first and greatest of the naturalists,’ as Dr. Wollaston styles Wallace, visited New Guinea, it was ‘the first time that any European had ventured to reside alone and practically unprotected on the mainland of this country,’ which, dangerous as it is now in the same regions, was infinitely more so then. Of the journals of his voyagings, ‘The Malay Archipelago’ will always be ranked among the greatest narratives of travel. The fact that this volume has gone through a dozen editions is witness to its extraordinary popularity among intelligent minds, and hardly supports the belief that his scientific work has been forgotten. Nor can this popularity be a matter of much surprise, for few travellers have possessed Wallace’s powers of exposition, his lucidity and charm of style. Professor Strasburger of Bonn has declared that through ‘The Malay Archipelago’ ‘a new world of scientific knowledge’ was unfolded before him. ‘I feel it . . . my duty,’ he adds, ‘to proclaim it with gratitude.’ Wallace’s narrative has attracted during the past half-century numerous naturalists to follow in his tracks, many of whom have reaped rich aftermaths of his harvest; but certain it is that no explorer in the same, if in any other, region has approached his eminence, or attained the success he achieved.

“As a systematic zoologist, Wallace took no inconsiderable place; his *métier*, however, was different. He described, nevertheless, large sections of his Lepidoptera and of his birds, on which many valuable papers are printed in the *Transactions* of the learned societies and in various scientific periodicals. Of the former, especial mention may be made of that on variation in the ‘Papilionidæ of the Malayan Region,’ of which Darwin has recorded: ‘I have never in my life been more struck by any paper.’ Of the latter reference may be drawn to his account of the ‘Pigeons of the Malay Archipelago’ and his paper on the ‘Passerine Birds,’ in which he proposed an important new arrangement of the families of that group (used later in his ‘Geographical Distribution’) based on the feathering of their wings.

Without a lengthy search through the zoological records, it would be impossible to say how many species Wallace added to science; but the constant recurrence in the Catalogue of Birds in the British Museum of 'wallacei' as the name bestowed on various new species by other systematists, and of 'Wallace' succeeding those scientifically named by himself, is an excellent gauge of their very large number.

"In the field of anthropology Wallace could never be an uninterested spectator. He took a deep interest, he tell us, in the study of the various races of mankind. His accounts of the Amazonian tribes suffered greatly by the loss of his journals; but of the peoples of the Malay Archipelago he has given us a most interesting narrative, detailing their bodily and mental characteristics, and showing how their distribution accorded with that of the fauna on the opposite sides—Malays to the West, Papuans to the East—of Wallace's Line. If fuller investigation of the New Guinea tribes requires some modification in regard to their origin, his observations, as broadly outlined then, remain true still. His opinions on the origin of the Australian aborigines—that they were a low and primitive type of Caucasian race—which, when first promulgated, were somewhat sceptically received, are now those accepted by many very competent anthropologists.

"Wallace's contributions to Geographical Science were only second in importance to those he so pre-eminently made to biology. Though skilled in the use of surveying instruments, he did little or no map-making—at all times a laborious and lengthy task—for, with more important purposes in his mind, he could not spare the time, nor did the limitations to his movements permit any useful attempt. Yet he did pure geographical work quite as important. The value of the comparative study of the flora and fauna of neighbouring regions, the great differences in the midst of much likeness between the organic life of neighbouring land masses, was a subject that was always in Wallace's mind during his exploration of the Amazon Valley, for he perceived that the physical geography and the distribution of these animals and plants were of the greatest service in elucidating their history where the geological record was defective. As is well known, the visual inspection of the geological structure of tropical countries is always difficult and often impossible to

make out because of the dense vegetation upon the surface and even the faces of the river gorges. But for the loss of his collections and notes we should have had from Wallace's pen a *Physical History of the Amazon*. This loss was, however, amply made up by his very original contributions to the geography of the Malay Archipelago. 'The Zoological Geography of the Malay Archipelago' and 'The Physical Geography of the Malay Archipelago' (written on Eastern soil, with the texts of his discourses around him) were the forerunners of his monumental 'Geographical Distribution of Animals,' elaborated in England after his return. 'To the publication of the "Geographical Distribution of Animals" we owe the first scientific study of the distribution of organic life on the globe, which has broadened ever since, and continues to interest students daily; his brilliant work in Natural History and Geography . . . is universally honoured,' are the opinions of Dr. Scott speaking as President of the Linnean Society of London.

"One of Wallace's most important contributions to the physical geography of the Malay region was his discovery of the physical differences between the western and the eastern portions of the Archipelago; i.e. that the islands lying to the east of a line running north from the middle of the Straits of Bali and outside Celebes were fragments of an ancient and larger Australian continent, while those to the western side were fragments of an Asiatic continent. This he elucidated by recognising that the flora and fauna on the two sides of the line, close though these islands approached each other, were absolutely different and had remained for ages uncommingled. This line was denominated 'Wallace's Line' by Huxley, and this discovery alone would have been sufficient to associate his name inseparably with this region of the globe."—H. O. F.

Like Darwin, Wallace gave excessive attention to the suggestions and criticisms of people who were obviously ignorant of the subjects about which they wrote. He was never impatient with honest ignorance or considered the lowly position of his correspondents. He replied to all letters of inquiry (and he received many from working men), and always gave his best knowledge and advice to anyone who desired it. There was not the faintest suggestion of the despicable sense of superiority about him.

"I had, of course, revelled in 'The Malay Archipelago' when a boy," says Prof. Cockerell, "but my first personal relations with Dr. Wallace arose from a letter I wrote him after reading his 'Darwinism,' then (early in 1890) recently published. The book delighted me, but I found a number of little matters to criticise and discuss, and with the impetuosity of youth proceeded to write to the author, and also to send a letter on some of the points to *Nature*. I have possibly not yet reached years of discretion, but in the perspective of time I can see with confusion that what I regarded as worthy zeal might well have been characterised by others as confounded impudence. In the face of this, the tolerance and kindness of Dr. Wallace's reply is wholly characteristic: 'I am very much obliged to you for your letter containing so many valuable emendations and suggestions on my "Darwinism." They will be very useful to me in preparing another edition. Living in the country with but few books, I have often been unable to obtain the *latest* information, but for the purpose of the argument the facts of a few years back are often as good as those of to-day—which in their turn will be modified a few years hence. . . . You appear to have so much knowledge of details in so many branches of natural history, and also to have thought so much on many of the more recondite problems, that I shall be much pleased to receive any further remarks or corrections on any other portions of my book.' This letter, written to a very young and quite unknown man in the wilds of Colorado, who had merely communicated a list of more or less trifling criticisms, can only be explained as an instance of Dr. Wallace's eagerness to help and encourage beginners. It did not occur to him to question the propriety of the criticisms, he did not write as a superior to an inferior; he only saw what seemed to him a spark of biological enthusiasm, which should by all means be kindled into flame. Many years later, when I was at his house, he produced with the greatest delight some letters from a young man who had gone to South America and was getting his first glimpse of the tropical forest. What discoveries he might make! What joy he must have on seeing the things described in the letter, such things as Dr. Wallace himself had seen in Brazil so long ago!"

Wallace's critical faculty was always keen and vigilant. Un-

like some critics, however, he relished genuine and well-informed criticism of his own writings. Flattery he despised; whilst the charge of dishonesty aroused strongest resentment. Deceived he might be, but he required clear proof that his own eyes and ears had led him astray. Romanes, who had propounded the forgotten theory of physiological selection, charged Wallace with adopting it as his own. This was not only untrue, it was ridiculous; and Wallace, after telling him so and receiving no apology, dropped him out of his recognition. During Romanes' illness Mr. Thiselton-Dyer wrote to Wallace and sought to bring about a reconciliation, and Wallace replied:

Parkstone, Dorset. September 26, 1893.

My dear Thiselton-Dyer,—I am sorry to hear of Romanes' illness, because I think he would have done much good work in carrying out experiments which require the leisure, means and knowledge which he possesses. I cannot, however, at all understand his wishing to have any communication from myself. I do not think I ever met Romanes in private more than once, when he called on me more than twenty years ago about some curious psychical phenomena occurring in his own family; and perhaps half a dozen letters—if so many—may have passed between us since. There is therefore no question of personal friendship disturbed. I consider, however, that he made a very gross misstatement and personal attack on me when he stated, both in English and American periodicals, that in my "Darwinism" I adopted his theory of "physiological selection" and claimed it as my own, and that my adoption of it was "unequivocal and complete." This accusation he supported by such a flood of words and quotations and explanations as to obscure all the chief issues and render it almost impossible for the ordinary reader to disentangle the facts. I told him then that unless he withdrew this accusation as publicly as he had made it I should decline all future correspondence with him, and should avoid referring to him in any of my writings.

This is, of course, very different from any criticism of my theories; that, or even ridicule, would never disturb me; but when a man has made an accusation of literary and scientific dishonesty, and has done all he can to spread this accusation over the whole civilised world, my only answer can be—after

showing, as I have done (*see Nature*, vol. xliii., pp. 79 and 150), that his accusations are wholly untrue—to ignore his existence.

I cannot believe that he can want any sympathy from a man he says has wilfully and grossly plagiarised him, unless he feels that his accusations were unfounded. If he does so, and will write to me to that effect (for publication, if I wish, after his death), I will accept it as full reparation and write him such a letter as you suggest.—Believe me yours very faithfully,

ALFRED R. WALLACE.

SIR W. T. THISELTON-DYER TO A. R. WALLACE

Kew. September 27, 1897.

Dear Mr. Wallace,—I am afraid I have been rather guilty of an impertinence which I hope you will forgive.

Romanes is an old acquaintance of mine of many years' standing. Personally, I like him very much; but for his writings I confess I have no great admiration.

Pray believe me I had no mission of any sort on his part to write to you. But I feel so sorry for him that when he told me how much he regretted that he did not stand well with you, I could not resist writing to tell you of the calamities that have befallen him.

I must confess I was in total ignorance of what you tell me. I don't see how, under the circumstances, you can do anything. I was never more surprised in my life, in fact, than when I read your letter. The whole thing is too childishly preposterous.

Romanes laments over *me* because he says I wilfully misunderstand his theory. The fact is, poor fellow, that I do not think he understands it himself. If his life had been destined to be prolonged I should have done all in my power to have induced him to occupy himself more with observation and less with mere logomachy.

I cannot get him to face the fact that natural hybrids are being found to be more and more common amongst plants. At the beginning of the century it was supposed that there were some sixty recognisable species of willows in the British Isles: now they are cut down to about sixteen, and all the rest are resolved into hybrids.—Ever sincerely,

W. T. THISELTON-DYER.

Wallace was a seeker after Truth who was never shy of his august mistress, whatever robes she wore. "I feel within me," wrote Darwin to Henslow, "an instinct for truth, or knowledge or discovery, of something of the same nature as the instinct of virtue." This was equally true of Wallace. He had a fine reverence for truth, beauty and love, and he feared not to expose error. He paid no respect to time-honoured practices and opinions if he believed them to be false. Vaccination came under his searching criticism, and in the face of nearly the whole medical faculty he denounced it as quackery condemned by the very evidence used to defend it. He very carefully examined the claims of phrenology, which had been laughed out of court by scientific men, and he came to the conclusion that "in the present (twentieth) century phrenology will assuredly attain general acceptance. It will prove itself to be the true science of the mind. Its practical uses in education, in self-discipline, in the reformatory treatment of criminals, and in the remedial treatment of the insane, will gain it one of the highest places in the hierarchy of the sciences; and its persistent neglect and obloquy during the last sixty years of the nineteenth century will be referred to as an example of the almost incredible narrowness and prejudice which prevailed among men of science at the very time they were making such splendid advances in other fields of thought and discovery."¹

Wallace was not even scared out of his wits by ghosts, for, unlike Coleridge, he believed in them although he thought he had seen many. Whether truth came from the scaffold or the throne, the séance or the sky, it did not alter the truth, and did not prejudice or overbear his judgment. He shed his early materialism (which temporarily took possession of him as it did of many others as a result of the shock following the overwhelming discoveries of that period) when he was brought face to face with the phenomena of the spiritual kingdom which withstood the searching test of his keen observation and reasoning powers. Prejudices, preconceived notions, respect for his scientific position or the opinions of his eminent friends or the reputation of the learned societies to which he belonged—all were quietly and firmly put aside when he saw what he recognised to be

¹ "The Wonderful Century," p. 437.

the truth. If his fellow-workers did not accept it, so much the worse for them. He stood four-square against the onslaught of quasi-scientific rationalism, which once threatened to obliterate all the ancient landmarks of morality and religion alike. He made mistakes, and he admitted and corrected them, because he verily loved Truth for her own sake. And to the very end of his long life he kept the windows of his soul wide open to what he believed to be the light of this and other worlds.

He was, then, a man of lofty ideals, and his idealism was at the base of his opposition to the materialism which boasted that Natural Selection explained all adaptation, and that Physics could give the solution of Huxley's poser to Spencer: "Given the molecular forces in a mutton chop, deduce Hamlet and Faust therefrom," and which regarded mind as a quality of matter as brightness is a quality of steel, and life as the result of the organisation of matter and not its cause.

"We have ourselves," wrote Prof. H. F. Osborn in an account of Wallace's scientific work which Wallace praised, "experienced a loss of confidence with advancing years, an increasing humility in the face of transformations which become more and more mysterious the more we study them, although we may not join with this master in his appeal to an organising and directing principle." But profound contemplation of nature and of the mind of man led Wallace to belief in God, to accept the Divine origin of life and consciousness, and to proclaim a hierarchy of spiritual beings presiding over nature and the affairs of nations. "Whatever," writes Dr. H. O. Forbes, "may be the last words on the deep and mysterious problems to which Wallace addressed himself in his later works, the unquestioned consensus of the highest scientific opinion throughout the world is that his work has been for more than half a century, and will continue to be, a living stimulus to interpretation and investigation, a fertilising and vivifying force in every sphere of thought."

It is perhaps unprofitable to go further than in previous chapters into his so-called heresies—political, scientific or religious. Yet we may imitate his boldness and ask whether he was not, perhaps, in advance of his age and whether his heresies were not shrewd anticipations of some truth at present but partially revealed. Take the example of Spiritualism, which, I suppose, has more opponents than anti-vaccination. No one can over-

look the fact that Spiritualism has many scientific exponents—Myers, Crookes, Lodge, Barrett and others. Prejudices against Spiritualism are as unscientific as the credulity which swallows the mutterings of every medium. Podmore's two ponderous volumes on the History of Spiritualism are marred by an obvious anxiety to make the very least, if not the very worst, of every phenomenon alleged to be spiritualistic. That kind of deliberate and obstinate blindness which prided itself on being the clear cold light of science Wallace scorned and denounced. He did not insist upon spiritualistic manifestations shaping themselves according to his own predesigned moulds in order to be investigated. He watched for facts whatever form they assumed. He fully recognised that the phenomena he saw and heard could be easily ridiculed, but behind them he as fully believed that he came into contact with spiritual realities which remain, and which led him to other explanations of the higher faculties of man and the origin of life and consciousness than were acceptable to the materialistic followers of Haeckel, Büchner and Huxley. And who dares dogmatically to assert in the name of science and in the second decade of the twentieth century, when the deeper meanings of evolution are being revealed, and the philosophy of Bergson is spoken about on the house-tops, that he was wrong? In these views may he not become the peer of Darwin?

At first blush it may seem to be a bad example of special pleading to attempt to discover the reason for his opposition to vaccination in his idealism. But it is not far from the truth. He believed in a Ministry of Public Health, that doctors should be servants of the State, and that they should be paid according as they kept people well and not ill. Health is the natural condition of the human body when it is properly sustained and used. And chemicals, even in sickness, are of less importance than fresh air, light and proper food. He ridiculed, too, the notion of unhealthy places. "It is like," he wrote to Mr. Birch, "the old idea that every child must have measles, and the sooner the better." To the same correspondent, who was contemplating going into virgin forests and who expressed his fear of malaria, he replied: "There is no special danger of malaria or other diseases in a dense forest region. I am sure this is a delusion, and the dense virgin forests, even when swampy, are, in a state

of nature, perfectly healthy to live in. It is man's tampering with them, and man's own bad habits of living, that render them unhealthy. Having now gone over all Spruce's journals and letters during his twelve years' life in and about the Amazonian forests, I am sure this is so. And even where a place is said to be notoriously 'malarious,' it is mostly due not to infection only, but to predisposition due to malnutrition or some bad mode of living. A person living healthily may, for the most part, laugh at such terrors. Neither I nor Spruce ever got fevers when we lived in the forests and were able to get wholesome food." "Health," he said to the present writer, "is the best resistant to disease, and not the artificial giving of a mild form of a disease in order to render the body immune to it for a season. Vaccination is not only condemned upon the statistics which are used to uphold it, but it is a false principle—unscientific, and therefore doomed to fail in the end." Besides which, he believed in mental healing, and had recorded definite and certain benefit from spiritual "healers." And he reminded himself that amongst doctors (witness the blind opposition encountered by Lister's discoveries) were found from time to time not a few enemies of the true healing art, and obstinate defenders of many forms of quackery.

Wallace made no claim to be an original investigator. He knew his limitations, and said again and again that he could not have conducted the slow and minute researches or have accumulated the vast amount of detailed evidence to which Darwin, with infinite patience, devoted his life. He was genuinely glad that it had not fallen to his lot to write "The Origin of Species." He felt that his chief faculty was to reason from facts which others discovered. Yet he had that original insight and creative faculty which enabled him to see, often as by flashlight, the explanation which had remained hidden from the eyes of the man who was most familiar with the particular facts, and he elaborated it with quickening pulse, anxious to put down the whole conception which filled his mind lest some portion of it should escape him. Therein lay one secret of his great genius. He often said that he was an idler, but we know that he was a patient and industrious worker. His idleness was his way of describing his long musings, waiting the bidding of her whom God inspires—Truth, who often hides her face from the clouded

eyes of man. For hours, days, weeks, he was disinclined to work. He felt no constraining impulse, his attention was relaxed or engaged upon a novel, or his seeds, or the plan of a new house, which always excited his interest. Then, apparently suddenly, whilst in one of his day dreams, or in a fever (as at Ternate, to recall the historical episode when the theory of Natural Selection struck him), an explanation, a theory, a discovery,¹ the plan of a new book, came to him like a flash of light, and with the plan the material, the arguments, the illustrations; the words came tumbling one over the other in his brain, and as suddenly his idleness vanished, and work, eager, prolonged, unwearying, filled his days and months and years until the message was written down and the task fully accomplished. Whilst writing he referred to few books, but wrote straight on, adding paragraph to paragraph, chapter to chapter, without recasting or revision.² And the result was fresh, striking, original. It was a creation. The work being done, he relapsed into his busy idleness. The truth, as he saw it, seemed to come to him. Some people called him a prophet, but he was not conscious of that high calling. I do not remember him saying that he was only a messenger. Perhaps later, when he was reviewing his life, he connected his sudden inspirations with a higher source, but for their realisation he relied upon a foundation of veritable facts, facts patiently accumulated, a foundation laid broad and deep. He had the vision of the prophet allied with the wisdom of the philosopher and the calm mental detachment of the man of science. Perhaps another explanation of his genius may be found in his open-mindedness. Truth found ready

¹ "I have been speculating last night," wrote C. Darwin to his son Horace, "what makes a man a discoverer of undiscovered things; and a most perplexing problem it is. Many men who are very clever—much cleverer than the discoverers—never originate anything. As far as I can conjecture, the art consists in habitually searching for the causes and meaning of everything which occurs."—"Emma Darwin," p. 207.

² It is interesting to compare this with Darwin's manner of writing. Darwin confessed: "There seems to be a sort of fatality in my mind leading me to put at first my statement or proposition in a wrong or awkward form. Formerly I used to think about my sentences before writing them down; but for several years I have found that it saves time to scribble in a vile hand whole pages as quickly as I possibly can, contracting half the words; and then correct deliberately. Sentences thus scribbled down are often better ones than I could have written deliberately."

access to his conscience, and always a warm welcome, and he saw with open eyes where others were stone-blind.

He belonged to our common humanity. No caste or acquired pride or unapproachable intellectualism cut him off from the people. His simple humanness made him one with us all. And his humanity was singularly comprehensive. It led him, for instance, to investigate the subject of suffering in animals. He noticed that all good men and women rightly shrank from giving pain to them, and he set himself to prove that the capacity for pain decreased as we descended the scale of life, and that poets and others were mistaken when they imputed acute suffering to the lower creation, because of the very restricted response of their nervous system. Even in the case of the human infant, he concluded that only very slight sensations are at first required, and that such only are therefore developed. The sensation of pain does not, probably, reach its maximum till the whole organism is fully developed in the adult individual. "This," he added, with that characteristic touch which made him kin to all oppressed people, "is rather comforting in view of the sufferings of so many infants needlessly sacrificed through the terrible defects of our vicious social system."

To Wallace pain was the birth-cry of a soul's advance—the stamp of rank in nature is capacity for pain. Pain, he held, was always strictly subordinated to the law of utility, and was never developed beyond what was actually needed for the protection and advance of life. This brings the sensitive soul immense relief. Our susceptibility to the higher agonies is a condition of our advance in life's pageant.

Take another instance. Amongst his numerous correspondents there were not a few who decided not to take life, for food, or science, or in war. One young man who went out with the assistance of Wallace to Trinidad and Brazil to become a naturalist, and to whom he wrote many letters¹ of direction and encouragement, gave up the work of collecting—to Wallace's sincere disappointment—and came home because he felt that it was wrong to take the lives of such wondrous and beautiful birds and insects. Another correspondent, who had joined the Navy, wrote a number of long letters to Wallace setting forth his con-

¹ See pp. 451, 457.

scientific objections to killing, arrived at after reading Wallace's books; and although Wallace endeavoured from prudential considerations to restrain him from giving up his position, he nevertheless wholly sympathised with him and in the end warmly defended him when it was necessary to do so. The sacrifice, too, of human life in dangerous employments for the purpose of financial gain, no less than the frightful slaughter of the battlefield, was abhorrent to Wallace and aroused his intensest indignation. Life to him was sacred. It had its origin in the spiritual kingdom. "We are lovers of nature, from 'bugs' up to 'humans,'" he wrote to Mr. Fred Birch.

By every means he laboured earnestly to secure an equal opportunity of leading a useful and happy life for all men and women. He championed the cause of women—of their freer life and their more active and public part in national service. He found the selective agency, which was to work for the amelioration he desired, in a higher form of sexual selection, which will be the prerogative of women; and therefore woman's position in the not distant future "will be far higher and more important than any which has been claimed for or by her in the past." When political and social rights are conceded to her on equality with men, her free choice in marriage, no longer influenced by economic and social considerations, will guide the future moral progress of the race, restore the lost equality of opportunity to every child born in our country, and secure the balance between the sexes. "It will be their (women's) special duty so to mould public opinion, through home training and social influence, as to render the women of the future the regenerators of the entire human race."

He was acutely anxious that his ideals should be realised on earth by the masses of the people. He had a large and noble vision of their future. And he had his plan for their immediate redemption—national ownership of the soil, better housing, higher wages, certainty of employment, abolition of preventable diseases, more leisure and wider education, not merely for the practical work of obtaining a livelihood but to enable them to enjoy art and literature and song. His opposition to Eugenics (to adopt the word introduced by Galton, which Wallace called jargon) sprang from his idealism and his love of the people, as well as from his scientific knowledge. On the social side he

thought that Eugenics offered less chance of a much-needed improvement of environment than the social reforms which he advocated, whilst on the scientific side he believed that the attempt, with our extremely limited knowledge, to breed men and women by artificial selection was worse than folly. He feared that, as he understood it, Eugenics would perpetuate class distinctions, and postpone social reform, and afford quasi-scientific excuses for keeping people "in the positions Nature intended them to occupy," a scientific reading of the more offensive saying of those who, having plenty themselves, believe that it is for the good of the lower classes to be dependent upon others. "Clear up," he said to the present writer one day, when we drifted into a warm discussion of the teachings of some Eugenists; "change the environments so that all may have an adequate opportunity of living a useful and happy life, and give woman a free choice in marriage; and when that has been going on for some generations you may be in a better position to apply whatever has been discovered about heredity and human breeding, and you may then know which are the better stocks."

"Segregation of the unfit," he remarked to an interviewer after the Eugenic Conference, at which much was unhappily said that wholly justified his caustic denunciation, "is a mere excuse for establishing a medical tyranny. And we have enough of this kind of tyranny already . . . the world does not want the eugenicist to set it straight. . . . Eugenics is simply the meddling interference of an arrogant scientific priesthood."

Thus his radicalism and his so-called fads were born of his high aspirations. He was not the recluse calmly spinning theories from a bewildering chaos of observations, and building up isolated facts into the unity of a great and illuminating conception in the silence and solitude of his library, unmindful of the great world of sin and sorrow without. He could say with Darwin, "I was born a naturalist"; but we can add that his heart was on fire with love for the toiling masses. He had felt the intense joy of discovering a vast and splendid generalisation, which not only worked a complete revolution in biological science, but has also illuminated the whole field of human knowledge. Yet his greatest ambition was to improve the cruel conditions under which thousands of his fellow-creatures suffered and died, and to make their lives sweeter and happier. His mind was great

enough and his heart large enough to encompass all that lies between the visible horizons of human thought and activity, and even in his old age he lived upon the topmost peaks, eagerly looking for the horizon beyond. In the words of the late Mr. Gladstone, he "was inspired with the belief that life was a great and noble calling; not a mean and grovelling thing that we are to shuffle through as we can, but an elevated and lofty destiny."

But we must not be tempted into further disquisition. As he grew older the public Press as well as his friends celebrated his birthdays. Congratulations by telegram and letter poured in upon him and gave him great pleasure. Minor poets sang special solos, or joined in the chorus. One example may be quoted:

ALFRED RUSSEL WALLACE

8TH JANUARY, 1911

A little cot back'd by a wood-fring'd height,
 Where sylvan Usk runs swiftly babbling by:
 Here thy young eyes first look'd on earth and sky,
 And all the wonders of the day and night;
 O born interpreter of Nature's might,
 Lord of the quiet heart and seeing eye,
 Vast is our debt to thee we'll ne'er deny,
 Though some may own it in their own despite.
 Now after fourscore teeming years and seven,
 Our hearts are jocund that we have thee still
 A refuge in this world of good and ill,
 When evil triumphs and our souls are riv'n;
 A friend to all the friendless under heav'n;
 A foe to fraud and all the lusts that kill.

O champion of the Truth, whate'er it be!
 World-wanderer over this terrestrial frame;
 Twin-named with Darwin on the roll of fame;
 This day we render homage unto thee;
 For in thy steps o'er alien land and sea,
 Where life burns fast and tropic splendours flame,
 Oft have we followed with sincere acclaim
 To mark thee unfold Nature's mystery.
 For this we thank thee, yet one thing remains
 Shall shrine thee deeper in the heart of man,

In ages yet to be when we are dust;
Thou has put forth thy hand to rend our chains,
Our birthright to restore from feudal ban;
O righteous soul, magnanimous and just!

W. BRAUNSTON JONES.

Sir William Barrett, one of Wallace's oldest friends, visited him during the last year of his life, and thus describes the visit:

"In the early summer of 1913, some six months before his death, I had the pleasure of paying another visit and spending a delightful afternoon with my old friend. His health was failing, and he sat wrapped up before a fire in his study, though it was a warm day. He could not walk round his garden with me as before, but pointed to the little plot of ground in front of the French windows of his study—where he had moved some of his rarer primulas and other plants he was engaged in hybridising—and which he could just manage to visit. His eyesight and hearing seemed as good as ever, and his intellectual power was undimmed. . . .

"Dr. Wallace then, pointing to the beautiful expanse of garden, woodland and sea which was visible from the large study windows, burst forth with vigorous gesticulation and flashing eyes: 'Just think! All this wonderful beauty and diversity of nature results from the operation of a few simple laws. In my early unregenerate days I used to think that only material forces and natural laws were operative throughout the world. But these I now see are hopelessly inadequate to explain this mystery and wonder and variety of life. I am, as you know, absolutely convinced that behind and beyond all elementary processes there is a guiding and directive force; a Divine power or hierarchy of powers, ever controlling these processes so that they are tending to more abundant and to higher types of life.'

"This led Dr. Wallace to refer to my published lecture on 'Creative Thought' and express his hearty concurrence with the line of argument therein; in fact he had already sent me his views, which, with his consent, I published as a postscript to that lecture.

"Then our conversation turned upon recent political events, and it was remarkable how closely he had followed, and how heartily he approved, the legislation of the Liberal Govern-

ment of the day. His admiration for Mr. Lloyd George was unfeigned. 'To think that I should have lived to see so earnest and democratic a Chancellor of the Exchequer!' he exclaimed, and he confidently awaited still larger measures which would raise the condition of the workers to a higher level; and nothing was more striking than his intense sympathy with every movement for the relief of poverty and the betterment of the wage-earning classes. The land question, we agreed, lay at the root of the matter, and land nationalisation the true solution. In fact, ever since I read the proof-sheets of his book on this subject, which he corrected when staying at my house in Kingstown, I have been a member of the Land Nationalisation Society, of which he was President.

"Needless to say, Dr. Wallace was an ardent Home Ruler and Free Trader,¹ but on the latter question he said there should be an export duty on coal, especially the South Wales steam coal, as our supply was limited and it was essential for the prosperity of the country—and 'the purchaser pays the duty,' he remarked. I heartily agreed with him, and said that a small export duty *had* been placed on coal by the Conservative Government, but subsequently was removed. This he had forgotten, and when later on I sent him particulars of the duty and its yield, he replied saying that at that time he was so busy with the preparation of a book that he had overlooked the fact. He wrote most energetically on the importance of the Government being wise in time, and urged at least a 2s. export duty on coal.

"We talked about the question of a portrait of Dr. Wallace being painted and presented to the Royal Society, which had been suggested by the Rev. James Marchant, to whom Dr. Wallace referred, when talking to me, in grateful and glowing terms."—W. F. B.

Perhaps it should be added to Sir William Barrett's reminiscences that the movement which was set on foot to carry out this project was stayed by Wallace's death.

During the last years of his life his pen was seldom dry. His interest in science and in politics was fresh and keen to the closing week. He wrote "Social Environment and Moral

¹ But see *ante*, p. 390.

Progress" in 1912, at the age of 90. The book had a remarkable reception. Leading articles and illustrated reviews appeared in most of the daily newspapers. The book, into which he had put his deepest thoughts and feelings upon the condition of society, was hailed as a virile and notable production from a truly great man. After this was issued, he saw another, "The Revolt of Democracy," through the press. But this did not exhaust his activities. He entered almost immediately into a contract to write a big volume upon the social order, and as a side issue to help, as is mentioned in the Introduction, in the production of an even larger book upon the writings and position of Darwin and Wallace and the theory of Natural Selection as an adequate explanation of organic evolution. Age did not seem to weaken his amazing fertility of creative thought, nor to render him less susceptible to the claims of humanity, which he faced with a noble courage. In nobility of character and in magnitude, variety and richness of mind he was amongst the foremost scientific men of the Victorian Age, and with his death that great period, which was marked by wide and illuminating generalisations and the grand style in science, came to an end.

Apart altogether, however, from his scientific position and attainments, which set him on high, he was a noble example of brave, resolute, and hopeful endeavour, maintained without faltering to the end of a long life. And this is not the least valuable part of his legacy to the race.

When Henslow died, Huxley wrote to Hooker: "He had intellect to comprehend his highest duty distinctly, and force of character to do it; which of us dare ask for a higher summary of his life than that? For such a man there can be no fear in facing the great unknown; his life has been one long experience of the substantial justice of the laws by which this world is governed, and he will calmly trust to them still as he lays his head down for his long sleep." Let that also stand as the estimate of Wallace by his contemporaries, an estimate which we believe posterity will confirm. And to it we may add that death, which came to him in his sleep as a gentle deliverer, opened the door into the larger and fuller life into which he tried to penetrate and in which he firmly believed. If that faith be founded in truth, Darwin and Wallace, yonder as here, are united evermore.

I am writing these concluding words on the second anniversary of his death. Before me there lies the telegram which brought me the sad news that he had "passed away very peacefully at 9.25 a.m., without regaining consciousness." He was in his ninety-first year. It was suggested that he should be buried in Westminster Abbey, beside Charles Darwin, but Mrs. Wallace and the family, expressing his own wishes as well as theirs, did not desire it. On Monday, November 10th, he was laid to rest with touching simplicity in the little cemetery of Broadstone, on a pine-clad hill swept by ocean breezes. He was followed on his last earthly journey by his son and daughter, by Miss Mitten, his sister-in-law, and by the present writer. Mrs. Wallace, being an invalid, was unable to attend. The funeral service was conducted by the Bishop of Salisbury (Dr. Ridgeway), and among the official representatives were Prof. Raphael Meldola and Prof. E. B. Poulton representing the Royal Society; the latter and Dr. Scott representing the Linnean Society, and Mr. Joseph Hyder the Land Nationalisation Society. A singularly appropriate monument, consisting of a fossil tree-trunk from the Portland beds, has been erected over his grave upon a base of Purbeck stone, which bears the following inscription:

ALFRED RUSSEL WALLACE, O.M.

Born Jan. 8th, 1823, Died Nov. 7th, 1913

A year later, on the 10th of December, 1914, his widow died after a long illness, and was buried in the same grave. She was the eldest daughter of Mr. William Mitten, of Hurstpierpoint, an enthusiastic botanist, and in no mean degree she inherited her father's love of wild flowers and of the beautiful in nature. It was this similarity of tastes which led to her close intimacy and subsequent marriage, in 1866, with Wallace. Their married life was an exceedingly happy one. She was able to help him in his scientific labours, and she provided that atmosphere in the home life which enabled him to devote himself to his many-sided enterprises. And nothing would give him more joy than to know that this book is dedicated to her memory.

Soon after Wallace's death a Committee was formed (with Prof. Poulton as Chairman and Prof. Meldola as Treasurer) to erect a memorial, and the following petition was sent to the Dean and Chapter of Westminster Abbey:

"We, the undersigned, earnestly desiring a suitable national memorial to the late Alfred Russel Wallace, and believing that no position would be so appropriate as Westminster Abbey, the burial-place of his illustrious fellow-worker Charles Darwin, petition the Right Reverend the Dean and Chapter for permission to place a medallion in Westminster Abbey. We further guarantee, if the medallion be accepted, to pay the Abbey fees of £200.

ARCH. GEIKIE
WILLIAM CROOKES
A. B. KEMPE
E. RAY LANKESTER
D. H. SCOTT
D. PRAIN
A. E. SHIPLEY
RAPHAEL MELDOLA
P. A. MACMAHON

JOHN W. JUDD
OLIVER J. LODGE
E. B. POULTON
A. STRAHAN
H. H. TURNER
J. LARMOR
W. RAMSAY
SILVANUS P. THOMPSON
JOHN PERRY
JAMES MARCHANT
(Hon. Sec.)"

To which the Dean replied:

The Deanery, Westminster, S.W. December 2, 1913.

Dear Mr. Marchant,—I have pleasure in informing you that I presented your petition at our Chapter meeting this morning, and a glad and unanimous assent was accorded to it.

I should be glad later on to be informed as to the artist you are employing; and probably it would be as well for him and you and some members of the Royal Society to meet me and the Chapter and confer together upon the most suitable and artistic arrangement or rearrangement of the medallions of the great men of science of the nineteenth century.

Nothing could have been more satisfactory or impressive than the document with which you furnished me this morning. I hope to get it specially framed.—Yours sincerely,

HERBERT E. RYLE.

Mr. Bruce-Joy, who had made an excellent medallion of Dr. Wallace during his lifetime, accepted the commission to fashion the medallion for Westminster Abbey, and it was unveiled, by a happy but undesigned coincidence, on All Souls' Day, November 1, 1915, together with medallions to the memory of Sir Joseph Hooker and Lord Lister. In the course of his sermon, the Dean said—and with these words we may well conclude this book:

“To-day there are uncovered to the public view, in the North Aisle of the Choir, three memorials to men who, I believe, will always be ranked among the most eminent scientists of the last century. They passed away, one in 1911, one in 1912, and one in 1913. They were all men of singularly modest character. As is so often observable in true greatness, there was in them an entire absence of that vanity and self-advertisement which are not infrequent with smaller minds. It is the little men who push themselves into prominence through dread of being overlooked. It is the great men who work for the work's sake without regard to recognition, and who, as we might say, achieve greatness in spite of themselves.

“Alfred Russel Wallace was a most famous naturalist and zoologist. He arrived by a flash of genius at the same conclusions which Darwin had reached after sixteen years of most minute toil and careful observation. . . . It was a unique example of the almost exact concurrence of two great minds working upon the same subject, though in different parts of the world, without collusion and without rivalry. . . . Between Darwin and Wallace goodwill and friendship were never interrupted. Wallace's life was spent in the pursuit of various objects of intellectual and philosophical interest, over which I need not here linger. All will agree that it is fitting his medallion should be placed next to that of Darwin, with whose great name his own will ever be linked in the worlds of thought and science.

“All will acknowledge the propriety of these three great

names being honoured in this Abbey Church, even though it be, to use Wordsworth's phrase, already

“‘Filled with mementoes, satiate with its part
Of grateful England's overflowing dead.’

“These are three men whose lifework it was to utilise and promote scientific discovery for the preservation and betterment of the human race.”

APPENDIX

LISTS OF WALLACE'S WRITINGS

I.—BOOKS

DATE	TITLE
1853	"Palm Trees on the Amazon"
1853	"A Narrative of Travels on the Amazon and Rio Negro." New Edition in "The Minerva Library," 1889
1866	"The Scientific Aspect of the Supernatural"
1869	"The Malay Archipelago," 2 vols. Tenth Edition, 1 vol., 1890
1870	"Contributions to the Theory of Natural Selection." Republished, with "Tropical Nature," 1891
1874	"Miracles and Modern Spiritualism." Revised Edition, 1896
1876	"The Geographical Distribution of Animals," 2 vols.
1878	"Tropical Nature and other Essays." Printed in 1 vol. with "Natural Selection," 1891
1879	"Australasia." "Stanford's Compendium of Geography and Travel." (New Issue, 1893)
1880	"Island Life." Revised Edition, 1895
1882	"Land Nationalisation"
1885	"Bad Times"
1889	"Darwinism." 3rd Edition, 1901
1898	"The Wonderful Century." New Edition, 1903
1900	"Studies, Scientific and Social"
1901	"The Wonderful Century Reader"
1901	"Vaccination a Delusion"
1903	"Man's Place in the Universe." New Edition, 1904. Cheap 1s. Edition, 1912
1905	"My Life," 2 vols. New Edition, 1 vol., 1908
1907	"Is Mars Habitable?"
1908	"Notes of a Botanist on the Amazon and Andes," by Richard Spruce. Edited by A. R. Wallace
1910	"The World of Life"
1913	"Social Environment and Moral Progress"
1913	"The Revolt of Democracy"

II.—ARTICLES, PAPERS, REVIEWS, ETC.

The articles marked with an asterisk were republished in Wallace's "*Studies, Scientific and Social.*"

DATE	PERIODICAL OR SOCIETY	SUBJECT
	1850 <i>Proc. Zool. Soc., Lond.</i>	On the Umbrella Bird
	1852 " "	Monkeys of the Amazon
	1852-3 <i>Trans. Entomol. Soc.</i>	On the Habits of the Butterflies of the Amazon Valley
	1853 <i>Zoologist</i>	On the Habits of the Hesperidæ
	1853 <i>Proc. Zool. Soc., Lond.</i>	On some Fishes allied to Gymnotus
June 6	1853 Entomolog. Soc.	On the Insects used for Food by the Indians of the Amazon
June 13	1853 Royal Geograph. Soc.	The Rio Negro
	1854-5 <i>Zoologist</i>	Letters from Singapore and Borneo
	1854-6 <i>Trans. Entomol. Soc.</i>	Description of a New Species of Ornithoptera
	1855 <i>Annals and Mag. of Nat. Hist.</i>	On the Ornithology of Malacca
	1855 <i>Journ. Bot.</i>	Botany of Malacca
Sept.	1855 <i>Zoologist</i>	The Entomology of Malacca
	1855 <i>Annals and Mag. of Nat. Hist.</i>	On the Law which has regulated the Introduction of New Species
	1856 " "	Some Account of an Infant Orang-Outang
Dec.	1856 " "	On the Orang-Outang or Mias of Borneo
	1856 " "	On the Habits of the Orang-Outang of Borneo
	1856 " "	Attempts at a Natural Arrangement of Birds
Nov. 22	1856 <i>Chambers's Journ.</i>	A New Kind of Baby
	1856 <i>Journ. Bot.</i>	On the Bamboo and Durian of Borneo
	1856 <i>Zoologist</i>	Observations on the Zoology of Borneo
	1856-8 <i>Trans. Entomol. Soc.</i>	On the Habits, etc., of a Species of Ornithoptera inhabiting the Aru Islands
Dec.	1856-9 " "	Letters from Aru Islands and from Batchian
	1857 <i>Annals and Mag. of Nat. Hist.</i>	Natural History of the Aru Islands
	1857 " "	On the Great Bird of Paradise
	1857 <i>Proc. Geograph. Soc.</i>	Notes of a Journey up the Sadong River
	1858 " "	On the Aru Islands
	1858 <i>Zoologist</i>	Note on the Theory of Permanent and Geographical Varieties
	1858 " "	On the Entomology of the Aru Islands
	1858-61 <i>Trans. Entomol. Soc.</i>	Note on the Sexual Differences in the Genus <i>Lomaptera</i>
	1859 <i>Annals and Mag. of Nat. Hist.</i>	Correction of an Important Error affecting the Classification of the <i>Psittacidæ</i>
	1859 <i>Proc. Linn. Soc. (iii. 45)</i>	On the Tendency of Varieties to Depart Indefinitely from the Original Type ¹
Oct.	1859 <i>Ibis</i>	Geographical Distribution of Birds
Dec.	1859 Entomolog. Soc.	Note on the Habits of Scolytidæ and Bostrichidæ
	1860 <i>Journ. Geograph. Soc.</i>	Notes of a Voyage to New Guinea
	1860 <i>Ibis</i>	The Ornithology of North Celebes
	1860 <i>Proc. Zool. Soc., Lond.</i>	Notes on <i>Semioptera wallacii</i>

¹ Wallace's section of the Darwin-Wallace Essay entitled "On the Tendency of Species to form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection."

DATE	PERIODICAL OR SOCIETY	SUBJECT
	1860 <i>Proc. Linn. Soc.</i> (iv. 172)	Zoological Geography of Malay Archipelago
	1861 <i>Ibis</i>	On the Ornithology of Ceram and Waigiou
	1861 "	Notes on the Ornithology of Timor
	1862 <i>Proc. and Journ. Geogr. Soc.</i>	On the Trade between the Eastern Archipelago and New Guinea and its Islands
	1862 <i>Proc. Zool. Soc., Lond.</i>	List of Birds from the Sula Islands
	1862 <i>Ibis</i>	On some New Birds from the Northern Moluccas
	1862 <i>Proc. Zool. Soc., Lond.</i>	Narrative of Search after Birds of Paradise.
	1862 " "	On some New and Rare Birds from New Guinea
	1862 " "	Description of Three New Species of <i>Pitta</i> from the Moluccas
	1863 <i>Annals and Mag. of Nat. Hist.</i>	On the Proposed Change in Name of <i>Gracula pectoralis</i>
	1863 <i>Entomol. Journ.</i>	Notes on the Genus <i>Iphia</i> s
	1863 <i>Ibis</i>	Note on <i>Corvus senex</i> and <i>Corvus fusci-capillus</i>
	1863 "	Notes on the Fruit-Pigeons of Genus <i>Treron</i>
	1863 <i>Intellectual Observer</i>	The Bucerotidæ, or Hornbills
	1863 <i>Proc. Zool. Soc., Lond.</i>	List of Birds collected on Island of Bouru
April	1863 <i>Zoologist</i>	Who are the Humming-Bird's Relations?
June	1863 <i>Royal Geograph. Soc.</i>	Physical Geography of the Malay Archipelago
	1863 <i>Proc. Zool. Soc., Lond.</i>	On the Identification of <i>Hirundo esculenta</i> , Linn.
	1863 " "	List of Birds inhabiting the Islands of Timor, Flores and Lombok
	1863 <i>Annals and Mag. of Nat. Hist.</i>	On the Rev. S. Haughton's Paper on the Bee's Cell and the Origin of Species
Jan. 1	1864 <i>Nat. Hist. Rev.</i>	Some Anomalies in Zoological and Botanical Geography
Jan. 7	1864 <i>Edinburgh New Journ. (Philos.)</i>	Ditto
	1864 <i>Proc. Zool. Soc., Lond.</i>	Parrots of the Malayan Region
	1864 <i>Anthropol. Soc. Journ.</i>	The Origin of Human Races and the Antiquity of Man deduced from Natural Selection
	1864 <i>Proc. Entom. Soc. and Zoologist</i>	Effect of Locality in producing Change of Form in Insects
	1864 <i>Proc. Entom. Soc.</i>	Views on Polymorphism
	1864 <i>Ibis</i>	Remarks on the Value of Osteological Characters in the Classification of Birds
	1864 "	Remarks on the Habits, Distribution, etc., of the Genus <i>Pitta</i>
	1864 "	Note on <i>Astur griseiceps</i>
	1864 <i>Nat. Hist. Rev.</i>	Bone Caves in Borneo
	1865 <i>Proc. Zool. Soc., Lond.</i>	List of the Land Shells collected by Mr. Wallace in the Malay Archipelago
Jan.	1865 <i>Trans. Ethnolog. Soc.</i>	On the Progress of Civilization in North Celebes
Jan.	1865 " "	On the Varieties of Man in the Malay Archipelago
	1865 <i>Proc. Zool. Soc., Lond.</i>	Descriptions of New Birds from the Malay Archipelago
June 17	1865 <i>Reader</i>	How to Civilise Savages *
Oct.	1865 <i>Ibis</i>	Pigeons of the Malay Archipelago
	1866 <i>Trans. Linn. Soc. (xxv.)</i> (Abstract in <i>Reader</i> , April, 1864)	On the Phenomena of Variation and Geographical Distribution as illustrated by Papilionidæ of the Malayan Region

DATE		PERIODICAL OR SOCIETY	SUBJECT
	1866	<i>Proc. Zool. Soc. Lond.</i>	List of Lepidoptera collected by Swinton at Takow, Formosa
	1866	<i>Proc. Entomol. Soc.</i>	Exposition of the Theory of Mimicry as explaining Anomalies of Sexual Variation
	1867	<i>Zoologist</i>	
	1867	<i>Intellectual Observer</i>	The Philosophy of Birds' Nests
Jan.	1867	<i>Quarterly Journ. of Sci.</i>	Ice-Marks in North Wales
April	1867	"	The Polynesians and their Migrations *
July	1867	<i>Westminster Rev.</i>	Mimicry and other Protective Resemblances among Animals
Sept.	1867	<i>Science Gossip</i>	Disguises of Insects
Oct.	1867	<i>Quarterly Journ. of Sci.</i>	Creation by Law
	1867	<i>Proc. Entomol. Soc.</i>	
	1868	<i>Trans. Entomol. Soc.</i>	A Catalogue of the Cetoniidæ of the Malayan Archipelago, etc.
Jan. 7	1868	<i>Ibis</i>	Raptorial Birds of the Malay Archipelago
	1868	<i>Trans. Entomol. Soc.</i>	On the Pieridæ of the Indian and Australian Regions
	1868	—	The Limits of Natural Selection applied to Man *
	1869	<i>Trans. Entomol. Soc.</i>	Note on the Localities given in the "Longicornia Malayana"
	1869	<i>Journ. of Travel and Nat. Hist.</i>	A Theory of Birds' Nests
April	1869	<i>Quarterly Rev.</i>	Reviews of Lyell's "Principles of Geology" (entitled "Geological Climates and Origin of Species")
	1869	<i>Macmillan's Mag.</i>	Museums for the People *
	1869	<i>Trans. Entomol. Soc.</i>	Notes on Eastern Butterflies (3 Parts)
	1870	<i>Brit. Association Report</i>	On a Diagram of the Earth's Eccentricity, etc.
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May 23	1871	<i>Entomolog. Soc.</i>	Address on Insular Faunas, etc.
	1871	"	The Beetles of Madeira and their Teachings *
Nov.	1871	—	Reply to Mr. Hampden's Charges
	1873	<i>Journ. Linnean Soc.</i>	Introduction to F. Smith's Catalogue of Aculeate Hymenoptera, etc.
Jan. 4	1873	<i>Times</i>	Spiritualism and Science
April	1873	<i>Macmillan's Mag.</i>	Disestablishment and Disendowment, with a Proposal for a really National Church of England *
Sept. 16	1873	<i>Daily News</i>	Coal a National Trust *
Dec.	1873	<i>Contemp. Rev.</i>	Limitation of State Functions in the Administration of Justice *
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April	1874	—	Review of W. Marshall's "Phrenologist amongst the Todas"
April	1874	—	Review of G. St. Clair's "Darwinism and Design"
	1874	<i>Ibis</i>	On the Arrangement of the Families constituting the Order Passeres
May	1876	<i>Academy</i>	Review of Mivart's "Lessons from Nature"
	1877	<i>Proc. Geograph. Soc.</i>	The Comparative Antiquity of Continents
July	1877	<i>Quarterly Journ. of Sci.</i>	Review of Carpenter's "Mesmerism and Spiritualism," etc.
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	1886	Claims of Labour Lectures	The Depression of Trade *
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Jan.	1892	<i>Arena</i>	Human Progress, Past and Future *
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	1897	"Forecasts of Coming Century"	Re-occupation of Land, Solution of the Unemployed Problem *
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	1899	<i>N. Y. Independent</i>	White Men in the Tropics *
	1900	<i>N. Y. Sun</i>	Evolution
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